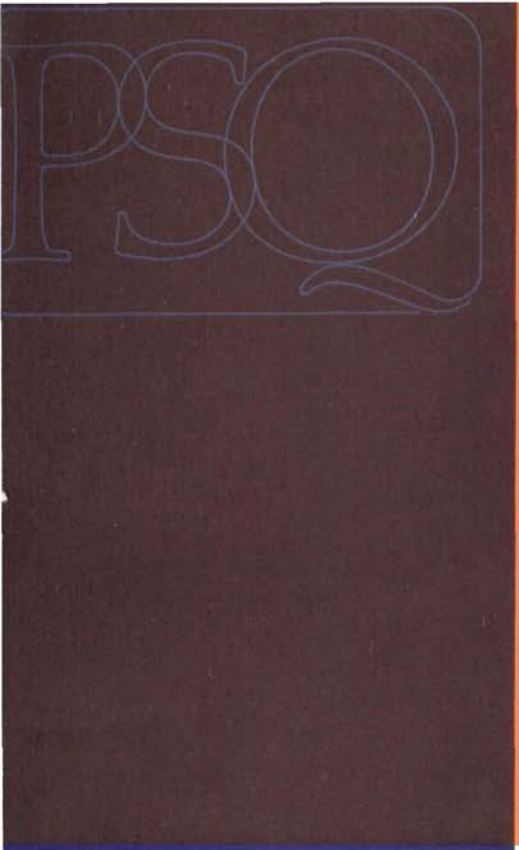

Political Science Quarterly

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Vol. LXXXVII No. 3 September 1972

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the Faculty of Political Science
Columbia University

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ROBERT L. ROTHSTEIN

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Reviews

In Memoriam: PHILIP EDWARD MOSELY

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Edited by the Faculty of Political Science of Columbia University, the POLITICAL SCIENCE QUARTERLY is published by The Academy of Political Science in March, June, September, and December.

Single issue, $2.50.

Printed by Capital City Press, Montpelier, Vermont 05602.
Second-class postage paid at New York, New York and additional mailing offices.

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THE ACADEMY OF POLITICAL SCIENCE | COLUMBIA UNIVERSITY
420 WEST 118TH STREET | NEW YORK, NEW YORK 10027

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Political Science Quarterly

Edited for the Academy of Political Science
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of Columbia University

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Library of Congress Catalog Card Number: 7-36315.

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On the Costs of Realism

ROBERT L. ROTHSTEIN
The Johns Hopkins University

Political Realism dominated the discipline of international relations throughout the 1950s. There were occasional complaints that it went "too far" or that it did not properly value some things which should be properly valued, but there was very little fundamental disagreement with its interpretation of what international politics was all about. Even the liberal Left joined the chorus, perhaps out of guilt for opposing rearmament against Hitler in the 1930s or perhaps for fear of being branded naive again in the face of a new threat.

Realism is, of course, no longer in style in academic circles. It has been under continuous attack for something over a decade—and the worst sort of attack, for it has simply been ignored as an anachronistic remnant of the discipline's early years. Its staying power, albeit in a less formal and coherent fashion, however, has been much greater and more persistent outside academia. This is not especially surprising, since there is normally a gap between the period when a doctrine is articulated in intellectual circles and accepted by men working in the field—and, of course, a gap between the period when it is rejected by the theorist and by the practitioner. The inconsistencies and the anomalies which trouble the theorist, and which make him more receptive to new doctrines, always appear less salient to men working on daily problems: the erosion of the

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woods is rarely immediately apparent to those who live amid the trees.

Whatever its current status, Realism reflects an important part of our recent intellectual heritage. The ease and pervasiveness of its dominance in the post-World War II years owes a great deal to the fact that it always seemed "natural" and "right" to practitioners —it encapsulated what they took for granted, especially after the failures of the 1930s and during the height of the cold war. In fact, Realism was something of an oddity as a theory, for it was always more attractive to practitioners than to theorists—the former were delighted to find that they were really "speaking" theory, but the latter were increasingly ambivalent about a doctrine which indiscriminately mixed normative, descriptive, and metaphysical statements. In any case, Realism was the doctrine which provided the intellectual frame of reference for the foreign policy establishment for something like twenty years, perhaps more. It did not determine the specific policy choices which they made, for no social theory has that power, but it did determine the categories by which they assessed the external world and the state of mind with which they approached prevailing problems. And that means that we cannot fully understand some of our recent disasters and dilemmas without understanding the vision of world politics which Realism made fashionable.

There have been innumerable discussions in recent years of the policy impact of various specific foreign policy doctrines which dominated the cold war years—for example, containment, the "Munich syndrome" (or "falling dominoes"), the proper manner of dealing with monolithic totalitarian states, and so on. None of these doctrines were in contradiction with one or another part of the Realist canon. However, they are not our primary concern, for this essay seeks some insight into a more fundamental level of professional belief patterns: not so much the "how" of dealing with the external world, but the "why" which determines what men within a common profession take for granted and what they think it is useful to ask about their problems and their commitments. It is especially important to get to this level of belief, for it persists beyond changes in any particular strategic doctrine. Those concerned with foreign affairs may change their views about containment or

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about intervention, but the alternatives they will consider, and the grounds on which they evaluate them, are heavily conditioned by their professional beliefs—which change much more slowly, if at all. Changes in the environment which alter strategic doctrines must be joined to changes in the manner of choosing and educating practitioners before we have a fundamental opportunity to reconstitute basic professional beliefs.

The Nature of Realism

Realism emerged in full flower in the years immediately after World War II. It was not a new doctrine. The first generation of Realists (apart from Machiavelli and Hobbes) appeared in the 1930s: note, for example, the writings of Nicholas Spykman, Reinhold Niebuhr, and E. H. Carr. For the most part, they were either ignored or accused of unwarranted cynicism.

The different reactions which Realism engendered in the 1930s and 1940s are only partly, if at all, attributable to the superior insights of postwar Realists like Hans Morgenthau and George Kennan. The prevailing way of attempting to solve the problems of world politics in the 1930s had proved increasingly inadequate. There is no point in once again recapitulating the failures of the "legal-rational" approach: it was obviously out of joint with a world which included several classic nonlegal, nonrational types. No twists and turns could transform Hitler into a Manchester businessman, and academic concentration on the law-abiding world of the future could not long survive a contemporary world of increasing tension and *sauve qui peut* philosophy.[1] Nonetheless, the new approach implicit in the Realist doctrine was not immediately or widely accepted. It probably requires a disaster of major proportion before the "shock of recognition" occurs and a new doctrine succeeds in thoroughly routing the incumbent. It thus took World War II to convert Realism from a rather esoteric and private critique of the behavior of the Western democracies into a popular and fully articulated interpretation of world politics, everywhere and any time.

[1] For an examination of the major strands of thought in the 1930s, see William T. R. Fox, "Interwar International Relations Research: The American Experience," *World Politics*, II (1949), 67-79.

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Why do men accept *any* doctrine or approach as legitimate? The answer hardly lies in methodological norms alone, for by themselves they are an insufficient response to the complexity of the world. An inevitably arbitrary and personal element underlies the choice. Perhaps the most an analyst can say, which may merely be another way of concealing his ignorance, is that the doctrine men choose to adopt has the power of making satisfactory sense of whatever it is that puzzles us. We cannot account for our choice by reason alone, nor by experience, since we "experience" only through the perspective of the doctrine in question. Polanyi's emphasis on the significance of the tacit components of men's knowledge and beliefs may provide the most valid interpretation: "We grant authority over ourselves to the conceptions which we have accepted because we acknowledge them as intimations—derived from the contact we make through them with reality—of an indefinite sequence of novel future occasions, which we may hope to master by developing these conceptions further, relying on our own judgment in its continued contact with reality."[2] In this sense, Realism was accepted because it seemed to have grasped the "natural order" of things in international politics and seemed to promise that emerging "puzzles" could be solved by reference to its standards.

Realism involved commitment to a set of propositions about international politics which were essentially extrapolations from the diplomatic history of nineteenth-century Europe. They were propositions which the generation of statesmen in Europe after 1919 either had lost or misunderstood: reeducation in the "perennials" was clearly necessary. The catechism was simple. All states sought, or would seek, power, given the opportunity. It was an essential prerequisite for the achievement of any other goals. Today's enemy could be tomorrow's ally (n.b., not "friend," for, as Salisbury put it, "Great Britain has no permanent friends, only permanent interests"). The use of any means was acceptable (atomic weapons created a dilemma, resolved by silence or metaphysical despair), or at least possible, though only one or two might be appropriate at any single moment. The best operator was the man who possessed "traditional wisdom"; and the man who possessed "traditional wisdom" was the best operator.

[2] Michael Polanyi, *Personal Knowledge* (New York, 1958), 104.

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The scenario and the stage directions are very familiar. The metaphor is deliberate, for many Realists considered international politics a great drama in which wise statesmen made "hard choices" in a bitter but limited struggle for dominance. They were constrained by their own power and their own fallibility, but at the very least they never fell victim to illusions about the "true" nature of the world. It was a world in which states were involved in an unending struggle with each other (because that was the nature of states in an anarchic world); power was necessary to survive in it or to continue to fight; all states were potential enemies (Realism requires enemies more than it needs friends), but the worst might be avoided by clever diplomacy and by virtue of the fact that all alike shared a similar conception of rational behavior.[3] It was indeed a dramatic picture, and an especially exciting one, for it was a drama of war in which the wartime mind predominated. This made it particularly attractive to an emerging generation of statesmen whose views had been formed as a response to the failure to stop Hitler before it was too late and who were thus predisposed toward a doctrine which would guarantee that the same errors would not be committed against Stalin.

The Realist model of world politics was simple and elegant. An image of states as billiard balls, interacting within a specific arena and according to established rules, became increasingly prevalent. Once the implication of the metaphor was grasped, that there are only a few immutable patterns of behavior in politics—billiard balls, after all, are not very complex phenomena—the principal preoccupation of statesmen became clear. They were to judge, by experience and intuition, the requisite amount of force necessary to move one or another ball in a preferred direction. Purposes, as in wartime where the need for survival and victory dominated everything, could be taken for granted. Individual idiosyncrasies, which might influence choice of purpose, or domestic politics, which might destroy the elegance of the game, could be safely ignored, for they were hardly significant in comparison to the external imperatives imposed by life in the international arena. All states would respond to the same drummer, irrespective of internal dif-

[3] For a different argument about rational behavior, which suffers from substantial ambiguities of its own, see Michael Oakeshott, *Rationalism in Politics* (New York, 1962), 81-109.

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ferences, because they had no choice if they wished to survive (at any rate, as a Great Power).

Is there something beyond its elegance and simplicity which has made this doctrine so popular, so to speak so "natural," to the practitioner? The power or fascination of a doctrine ultimately must rest on its apparent ability to provide answers to practical questions. The answers must be attributable to the doctrine, at least in the sense that some connection may safely be posited between successful practitioners and doctrinal commitment. In the case at hand, the ability to make that connection would imply that a substantive distinction exists between a Realist and a non-Realist.

Is it really that easy to distinguish a Realist from a non-Realist? The difficulty is that commitment to the Realists' image of world politics—a world scarred by a permanent quest for power by potentially wicked men—hardly guarantees realistic decisions about the practical world. Realists and non-Realists may disagree about the permanence of power as the decisive factor in international politics, but they can still reach similar judgments about specific cases. On the other hand, two confessed Realists may reach totally dissimilar conclusions about the same case—in fact, at times, it is difficult to relate an individual Realist's position on policy to his philosophical convictions. Correlating Morgenthau and Kennan on policy with Morgenthau and Kennan on "Realism" requires a Talmudist's skill and patience, not to say a willingness to suspend disbelief. The difficulty is that reality is so complex and ambiguous that the policies which we choose to call "realistic" at any particular moment depend to a significant degree on personal predispositions and perspectives.

What this suggests is that Realism involves something more than a temporal perspective on power and the nature of man. It also suggests that lists of characteristics presumably shared by all Realists are irrelevant: statesmen or analysts possessing all the characteristics can act very "unrealistically" (which we know only after the fact), while others possessing none of the characteristics may act "realistically" (which we also know only after the fact).[4] The more

[4] Kenneth Thompson, *Political Realism and the Crisis of World Politics* (Princeton, 1960), a glorification of the virtues of Realism, fails to provide any

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subtle contention that Realists share an awareness that full security is beyond attainment and that compromise and adjustment of interests are necessary, is more helpful. It implies that Realism involves a state of mind with which to approach problems, rather than the possession of a few characteristics or attachment to the permanent significance of a single operating principle.[5] Nevertheless, some groups have the same sense of the nature of politics and are not considered Realists (for example, some liberals). Moreover, the difficulty of discovering exactly why one policy position is more realistic than another persists.

Various efforts to give Realism an acceptable programmatic content have been inadequate, for the task itself is probably impossible. We can define a Realist arbitrarily as someone who possesses certain characteristics or who believes in certain doctrinal propositions, but there is no way in which we can convincingly relate those characteristics or beliefs to specific choices in the world of action. Realism simply constitutes belief in the wisdom of certain "eternal verities" about politics, conveniently collected in a few texts and conveniently "confirmed" by a series of all too recent blunders by non-Realists. The point surely is not that Realism is unimportant or irrelevant. But its real significance has not been in providing a (nonexistent) direct connection between theory and action. Its power and influence over the choice of specific actions has been—and perhaps remains—pervasive, but indirect. It has conditioned the political climate so that some actions seem "to stand to reason" and others seem naive—by definition. And it has furnished an authentic body of scripture to rationalize "hard choices," to justify the notion that a democratic foreign policy is inconceivable, and to provide psychic support for the acolyte compelled to lie in defense of his own interpretation of the national interest.

The great hero in the Realist canon has always been the successful diplomat—many of the founders and followers of Realism were frustrated Castlereaghs, or better yet, Metternichs—and the great

standard by which one would distinguish a realistic from an unrealistic action —unless we are to assume that policies which Mr. Thompson favors are, by definition, realistic.

[5] For this point, see William T. R. Fox, "Who are the Realists?" *Union Seminary Quarterly Review*, XXVI (1961), 345-48.

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danger the bumbling amateur. Professionals, after all, could always "work something out." The very ambiguity and uncertainty of the relationship between the theory and the choice of specific actions guarantee the supremity of the diplomat's role. What else but experience and intuition allow the necessary connections to be made? And who but the diplomat is trained (rather, one should say, "experienced") to make the necessary judgments? It is peculiarly true, then, that the lack of an obvious connection between the theory and a practical action, and the ensuing necessity of relying on a corps of skilled intermediaries, has made Realism singularly attractive to professional diplomats.

There are other themes—and omissions—which have made Realism attractive to the diplomatic mind. A sense of limitation, of the need to compromise and balance interests, has been one of the pervasive *leitmotifs* of the Realist doctrine. It means, above all, that one always has to be satisfied with a piece of the cake, that one's power begins to decline as its reach is extended. Probably no tenet in the doctrine is quite so attractive as this to the Realist practitioner. One need only consult the series of works produced by former members of the Policy Planning Staff in the Kennan and Nitze years: the great message which they wish to impress upon the American people concerns the limits of U. S. power and the imperfections of all solutions to major problems.[6] It perhaps also ought to be noted that there is a nostalgia in both doctrine and practitioner for a world in which the supremacy of foreign policy (and the diplomat as its high priest) could be taken for granted, and an underlying contempt for the manner in which democracies make foreign policy (none, for example, can resist quoting de Tocqueville's famous comments in this regard).

Realism presumes a world of similar states: it is a doctrine based upon, and beholden to, the behavioral styles of the traditional Great Power. Totalitarian, revolutionary, underdeveloped, and unstable states—as well as Small Powers, international organizations, and nongovernmental organizations like the multinational corporation or the Ford Foundation—are all unwelcome anomalies. Such

[6] See, for example, Charles Burton Marshall, *The Limits of Foreign Policy* (New York, 1954); and Louis J. Halle, *Civilization and Foreign Policy* (New York, 1952).

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states violate and perhaps destroy the notion of a shared, if tacit, sense of a range of permissible behavior for states.[7] It is not altogether inexplicable that many of the events which have surprised us—both theorists and practitioners—in the last twenty-five or so years have been perpetrated by these new kinds of states: the Nazi-Soviet pact in 1939, Pearl Harbor, the German blitz, Soviet acquisition of a nuclear capability, the Berlin blockade, North Korean aggression and Chinese intervention, Nasser's reaction to U. S. Aswan Dam "diplomacy," the sputniks, the Berlin wall, the installation of missiles in Cuba, and the more recent internal turmoil in both Indonesia and China. It is not that we failed to predict the exact moment or event; it is that we were neither politically nor psychologically prepared for them to happen at all.[8]

It is important to note that many of these failures resulted from the inability of men trained to deal with concrete contingencies as they arise to understand the actions of men or states committed to an ideological interpretation of world affairs—or at least to an interpretation not derived from the history of the European state system. In addition, a congenital bias against planning made it difficult to deal with those who did have a plan. At any rate, both Realists and practitioners shared a bias toward analyzing and evaluating the world according to habits and precepts drawn from European history.

One other aspect of Realism has made it especially attractive to diplomats and practitioners. Concentrating on interaction between states perceived as billiard balls tends to turn attention away from

[7] One of the fortuitous reasons for the predominance of Realism through the 1950s was the fact that the particular kind of world which it was trying to explain was bipolar, and predictability is much simpler in a bipolar world. Since the single opponent is by definition doing the worst everywhere, the only serious questions are specific and tactical: the "immutable" patterns are presumably especially immutable (if one pardons the expression) in a world in which conflict is assumed to be direct, pervasive, and unmediated.

[8] Obviously many factors account for each and all of these failures, and obviously there is no way in which a causal relationship can be proved between a particular doctrine and particular actions in the world. The point here is that in each of these cases it is at least arguable that one decisive factor may have been the failure of our conceptual framework to adequately prepare us for actions which were not that surprising (in retrospect). They were unusual or eccentric only within the context of the particular framework which was being employed.

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structural alterations in the international system itself. The systemic environment, in the large, is taken as a constant—that is, as a field fluctuating around a metaphorical balance of power. The result has been a static theory concerned only with creating or preserving an equilibrium. As such, only tactical questions—operator's questions about means, not ends—appear truly interesting. The central preoccupation is never why or where the system is going (it is going no place, by definition), but rather how to preserve the existing order of things. It has meant that the Realists have been very poor guides through the thickets of bipolarity, multipolarity, polycentrism, and the like.

Practitioners generally object strenuously to the notion that they all believe in any single doctrine. They point to the indisputable fact that there are sharp disagreements within the government over major issues like Vietnam and the ABM. This mistakes disagreements about specifics for disagreement about general attitudes and approaches. Anyone who reads the memoirs of former practitioners, or who spends any substantial amount of time talking with them, can attest to the existence of widely shared beliefs and very similar perceptions of what can be taken for granted about the conduct of foreign affairs. These shared beliefs and convictions are not held or expounded with anything like the formal elegance or coherence which one finds in a Morgenthau or Kennan text. Nonetheless, they exist and they reinforce—or repeat—the Realist canon. It may be violently unsettling to the political practitioner, but he does indeed "speak" theory—of a sort. It would be better for all of us if he were aware of it and understood what it implied.

The Practical Effects of Realism

The extent to which Realism has been elitist and antidemocratic was masked—or ignored—for many years, for the policies which dominated American foreign policy rested on a substantial domestic consensus about the proper way to deal with the Soviet and Chinese threats. Not only the mass public but anyone who disagreed with the conventional wisdom could be disregarded, be they reporters, professors, or "bleeding hearts" in general. What Realism passed on was a kind of romanticism about both policy—for the

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"responsibilities of power" meant that we had a stake as policeman or judge in anything happening anywhere—and the policymaker—who had to make "hard choices" in spite of domestic stupidity or indifference. The "professionals" would give Americans a good and prudent foreign policy even if they had to be tricked into it or misinformed or lied to. In effect, Realism has provided the high tone of necessity for a rather low range of behavior. In this sense, the revelation in the *Pentagon Papers* of a persistent disregard for the democratic process and a persistent fascination with fooling the press and obscuring the truth was entirely predictable.

Realism is also implicitly a conservative doctrine attractive to men concerned with protecting the status quo. It hardly predisposes its followers to look favorably at revolutionary change, for that kind of change threatens all the fences which Realism has erected: it means one might have to deal with some very untraditional states—and "diplomats"—about some very untraditional issues. It means that disagreement about ends and values might begin to creep into the system, surely an unfortunate development from the point of view of men committed to the notion that only the proper choice of means is ever really at issue.

From one point of view, Realism has always been an eminently sensible doctrine: its emphasis on the virtues of moderation, flexibility, and compromise was an intelligent response to the difficulties and dangers of living in an anarchic world. But from another point of view, Realism has emphasized the necessity for Great Powers to maintain their prestige, status, and credibility. Great Powers, by definition, are compelled to play "prestige politics," that is to say, a form of politics particularly difficult to compromise or control. Turning the other cheek could be disastrous, or at least imprudent, in a world dominated by the quest for power. In fact, it has always been necessary to use, or to appear to be willing to use, limited amounts of force quickly in order to avoid having to use larger amounts belatedly. This seemingly sensible proposition, so fundamental to a generation who remembered the follies of Chamberlain and Daladier, was very dangerous for men who could remember—or learn—nothing else. Flexibility, moderation, and compromise would have to take a back seat to the necessity of teaching the aggressors a lesson and enhancing the credibility of one's

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word. An awful lot of "brinksmanship" and waiting to see if "the other guy would blink first" could result.

Realism asserts—and it can be neither proved or disproved—that nothing much can be changed, that the only guide to the future is the past, and that the best interpreter of the way to get there safely is the operator skilled at negotiating limited compromises. It thus gives the "generalist," the operator armed only with traditional procedural skills, a central role in the conduct of foreign policy. But it is also a conservative and anti-innovative role; as a result, the doctrine has provided a kind of metaphysical justification for the passivity and procedural inertia of the Foreign Service and the State Department, characteristics already built into the policymaking system by incrementalism and the play of bureaucratic politics.

The nature of the role which the practitioner is expected to play also has had a crucial effect on the nature of the training he is expected to undergo. The only unanswered questions are tactical questions about applications. And there is no way to train someone to make correct tactical decisions except "on the job." Thus the proper training for the practitioner is never analytical or intellectual; presumably, his proper role is simply to apply known principles to individual cases. That task, which rests on a combination of experience, intuition, and familiarity with the latest details, can be learned only by doing—or, more accurately, by imitating. It is one of the few illustrations of a profession which takes anti-intellectualism as a virtue. In any case, it sharply circumscribes the ability of the practitioner to deal with untraditional events.

The fact that Realism has operated with a strikingly narrow definition of politics also has had a major effect on the behavior of its practitioners. Diplomatic maneuvering to achieve or maintain the gains of "high politics" became the norm—the analogy with the chessboard, an elegant and intricate arena of play, always seemed appropriate. New developments which undermine the utility of the analogy have to be either ignored or dismissed as irrelevant. Thus the State Department and its denizens have had little influence on a whole range of issues which have dominated foreign policy since World War II: for example, political and economic development in the underdeveloped countries, the relationship between nuclear weapons and political behavior, the control of the arms race, limited and sublimited war, and the erosion of the dis-

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tinction between foreign and domestic policy. It is misleading to assert, as some critics have done, that these are issues which have been *taken* from the State Department: it is more accurate to say that they have been given away in the apparent hope that they would disappear, or at least not intrude upon the ordered universe of diplomacy.

Realism has the ring of truth to it for men compelled to work in an environment which they can not always understand and can never adequately control. It provides a few simple keys which facilitate understanding (if only, inevitably, by oversimplification) and an intellectual justification for the failure to control (for all is unpredictable—although hardly unexpectable). None of this means that Realism has been responsible for, or "caused," any particular policy choice: it could just as well have been used, for example, to defend going into Vietnam as staying out. What it has done has been to foster a set of attitudes that predisposed its followers to think about international politics in a particularly narrow and ethnocentric fashion, and to set very clear bounds around the kinds of policies which it seemed reasonable to contemplate. And once decisions have been made, it has provided the necessary psychological and intellectual support to resist criticism, to persevere in the face of doubt, and to use any means to outwit or to dupe domestic dissenters.[9]

The Future of Realism

The appeal of Realism is deceptive and dangerous, for it rests on assumptions about state behavior which have become increasingly

[9] Realism also has had a profound impact on the relationship between the government and the academic world, but limitations of space preclude any extensive comment. In brief, since the principles of Realism were already known, *both* theorists and practitioners became preoccupied with narrow issues of policy: in effect, they competed with each other and threatened each other, rather than performed functions which were different but nonetheless supplemental. The failure of the academic world to perform one of its critical roles—questioning the conventional wisdom—merely reinforced the blocking power of the Realist syndrome. This is a serious charge, the significance of which has been ignored by both groups. It is not clear, unfortunately, that the academic doctrines which have replaced Realism are much of an improvement: we have moved from the academic-Realists' illegitimate intrusion into the realm of policy opinion to the methodologists' indifference to policy issues as beyond the state of the art. Neither position makes much sense.

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irrelevant. It treats one time-bound set of propositions as if they were universally applicable, and thus turns everyone's attention to problems of application—to issues of "how" not "why." It is always a doctrine of the powerful and for their survival. And it is always also a doctrine which takes for granted the primacy of foreign policy and the dominance of the security issue defined in terms of simple notions of power. It is, in sum, not only the classic version of a state-centric doctrine but also an affirmation of the rightful dominance of the Great Powers and the autonomy of their foreign policies.

We could treat this discussion as being of only historical interest but for one fact: despite Realism's increasing irrelevance as an interpretation of the external world, its hold over the mind of the practitioner is still formidable. Why this should be so can only be explained by the dominant—and thus exceedingly attractive—role which Realism assigns to the "generalist" practitioner (who gets a hunting license on all issues in spite of an absence of substantive expertise); and by the more general consideration that all doctrines persist at the practical level much beyond the point they begin to be assailed at the theoretical level. After all, for the practitioner to abandon or question what he considers to be his own particular expertise is to abandon or question the only thing which separates him from outsiders, and that is very threatening.

The greatest danger in this situation is that Realism is becoming even more irrelevant to the international system in the process of emerging. What we may be witnessing is the first systemic revolution occurring without the intervention of general war or the development of a wholly new kind of military technology. The central point is that the traditional security issue is no longer likely to be the dominant consideration in world politics. I am *very* far from asserting that security will no longer be an issue or that it will somehow disappear from the calculations of states—some analysts of the emerging system seem to take this position, at least implicitly, thus acting as if the realm of security and the realm of interdependence were in fact completely autonomous. It is clear, however, that security will be only one of the issues of world politics, albeit a crucial one, for it will have to share prominence with

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a range of issues heretofore left to technicians or to the play of domestic politics.

The growing interdependence on economic, social, and cultural matters within the state system obviously implies a system in which the autonomy and sovereignty of all the members—great and small—is being eroded. Rational decisionmaking on such issues requires a degree of international cooperation well beyond anything which has occurred in the field of security. (Even in NATO, for example, the United States always determined strategic questions by itself even though they affected all the allies.) This is particularly true because there is no guarantee that these issues will reduce the degree of conflict in the international system *unless* they are handled in a manner which is minimally satisfactory to all concerned. Interdependence clearly could just as well lead to trade wars and an insane effort to achieve autarchy as it could to increased prosperity and welfare; only a new style of decisionmaking and a change in basic thought patterns could turn these developments into an opportunity to enhance the degree of cooperation in the system. Finally, it deserves some mention that the security issue itself is becoming (or perhaps one should say, is finally being recognized as) increasingly one involving interdependence, as the recent agreements on the hot line and nuclear accidents attest. It will become even more so if nuclear weapons proliferate and arms technology itself continues to grow in complexity. Even a more mundane, but very critical, security issue like the control of conventional arms cannot be handled by any traditional formula—if it can be handled at all.

The attitudes and predispositions which Realism fosters constitute a classically inappropriate response to these developments. With its overly narrow conception of politics, and with its antiquated notions of sovereignty, Great Power dominance and the autonomy of foreign policy, the Realist response is bound to create conflict and destroy the possibility of working out new forms of cooperation. The potential which these issues have for creating either cooperation or conflict means that they must be deliberately manipulated to encourage cooperation. It may even be necessary to adopt a decisionmaking style borrowed from domestic politics, or

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to begin to take functions like planning seriously. We may also be compelled to contemplate other heresies. The Realist mentality would find it virtually impossible to even think about these matters in their proper dimension; worse yet, since Realism presupposes conflict, it is likely to turn the politics of interdependence into another exercise in the politics of security.

We cannot argue that the hold which Realism has maintained over the realm of practice is unimportant, for we can change nothing of substance until we first change fundamental patterns of professional beliefs. How we should—or can—do this is far from clear. Perhaps the most formidable obstacle to change is the absence of a generally acceptable alternative interpretation of world politics—men are unlikely to relinquish faith in one doctrine until a feasible alternative begins to be widely accepted. With a new doctrine, we could begin to educate future practitioners in a more sensible fashion. In the short-run, until we have rethought what we mean by "international relations," the best we can probably hope for is that continued discussion of these issues will at least compel the practitioner to think again about the wisdom of what he has for so long taken for granted. An academic social science which took the concerns of the practitioner more seriously and spent less time celebrating the procedural triumphs of methodology should be in the forefront of criticism of the old doctrine and the quest for a new one. These are slender needs on which to base much hope for reform, which surely explains why we seem to oscillate between radical proposals for reform, which have no possibility of implementation, and reassertions of the relevance of traditions which were obsolescent fifty years ago.

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Roosevelt and the Bonus Marchers of 1932

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The summer of 1932 was not especially uncomfortable, but for those of us working with Roosevelt it seemed very long. Matters without any obvious relation to campaign activities affected everything planned and done. They nagged our minds and demanded his attention. The immolation of Mayor Jimmy Walker was one of these; but there were others.

The most difficult of them had to do with the Depression, naturally, since it hung, a sort of miasma, over everything. For example, there were strange financial happenings in Europe; the mysteries of international banking that were plotted in directors' rooms of financial institutions were difficult for outsiders to understand and especially amateurs like ourselves. Then business failures and massive unemployment in Britain and on the continent were even more serious than our own. They were causing challenges to authority by shouting bands of black-shirts and brown-shirts orchestrated by demagogues, and governments were actually being overturned. More of these violently erupting volcanoes were reported almost daily.

We had no time to study these events carefully, but it was evident that they portended something sinister. Besides, they had a frightening similarity to occurrences at home, which had begun to have

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the same immediacy. The weakened economy had begun to collapse on men whose outcries were more readily listened to than the complaints of the smaller fellows who until then had suffered most. We ourselves watched with intense interest the daily and weekly reports from the centers of trade and the issuers of statistics, and every new indication was analyzed for its possible meaning. We made of it what we could and Roosevelt, widening his circle, took in masses of advice and information. For many of these developments he had no responsibility and so could not be blamed, but he was expected to have attitudes about them and, on occasion, to make appropriate pronouncements.

The worst were, of course, the simple ordeals of those who had to endure as best they could whatever anxieties and hardships the worsening times brought them. By this time, the millions who had no jobs were in an advanced stage of desperation. Summer was not so hard as winter; patched clothing and worn shoes did not have to keep out frost and slush, but food and shelter for a family were harder to provide. Private charities had practically exhausted their resources, and public agencies were rationing their meager appropriations.

One considerable difference was that the relentlessly progressing deterioration was squeezing many of those who had hitherto been immune or nearly so. Not only wages, but salaries too were being reduced when there were any jobs at all, and executives at the secondary levels were being put on part-time with the likely prospect of separation. In city after city, public employees were being asked to take salary cuts of 10 per cent or even more. In New York they were asked for a month's work without pay. People with debts faced settlements with no way to meet them. They had to resign themselves to foreclosures of pledged collateral, which might represent the savings of years, perhaps their business properties or homes. Graduates coming away from their campuses in May and June discovered that there were no jobs to be had and that there was not much they could do but join their fathers, who were sitting around a house only doubtfully theirs or an apartment whose landlord was demanding overdue rent.

It was this pushing upward of the crisis into income groups hitherto untouched that rang the alarm bells in offices and boardrooms and caused the foreboding that hung over superficially

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cheerful editorial pages in the newspapers. In spite of forced official optimism and patent playing down of bad news, there was a darkness closing over the nation that Americans had not the conditioning to endure. Were these the signs of collapse? Or were the few riotings and protest marches the forerunners of a revolution such as had tormented European nations? Not even a lively political campaign was likely to excite people hopelessly walking the streets, nor were promises for the future likely to bring much cheer to the homeless.

Quite suddenly, however, in July, Hoover became convinced that deflation had run its course and that an upward trend was starting. For a time his optimism seemed about to burst into jubilation. We ourselves were convinced, after an intensive study by Adolf Berle, that Hoover was relying on no more than a flurry in the stock market, a false indication of resumed activity. Frances Perkins hinted darkly that it might even have been induced. She spoke of those who could still afford to spend something substantial if it would stop the loose talk being reported to them of reform and reprisal. Just then, she was in a heated controversy with federal officials about the number of unemployed, having detected a kind of reporting that resulted in underestimation by some millions. Much to Roosevelt's amusement, her challenge had had fairly wide publicity. Her engagement was really with Hoover himself, who stood sponsor for the figures; and nothing could be better than to have the high priest of figures and reports discredited by an obscure New York State commissioner, and a woman at that. Roosevelt encouraged her; she was having much the best of the argument.

The continuing controversy over Depression phenomena was the background for everything else; but other events and controversies dragged along and refused to resolve themselves, further disturbing people's minds and adding to the prevalent confusion. Some were gradually concluded, but so many of those remaining were a discredit to Hoover and his supporters that we began to feel a furtive sympathy for the president so recently of high eminence and now so generally reviled.

There was a corresponding change in attitude toward Roosevelt. The general judgment that he was no great success as governor and that he offered no special promise of leadership was rapidly being revised. He was now being looked at more carefully; what he

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said and did was being watched for indications of his policies, and even his record was being reassessed. True, the *New York Times* was not yet satisfied with his pledges. To the editorialists they seemed neither frank nor comprehensive, and he continued to be criticized for being evasive. It was now generally understood, however, that he meant to do something about the prevalent misery, that he was well practiced in government, and that he had a more sophisticated understanding of economic problems and a more studied approach than had been imagined. This turn had taken place even before he had had much of anything to say except in the mutilated acceptance speech. Regard for him seemed to spread in some mysterious fashion; but that it was spreading, those who were watching as closely as we were could not doubt.

We guessed that the change was not really traceable to any efforts Roosevelt himself had made to impress the public with his prescience. He had hardly begun that effort and was, in fact, in no position to make curative suggestions. He was still uncertain. The improvement was owed more to a reassessment of his energy and intelligence. Now that he was being watched, he was seen to be a vigorous, mature, and competent public man, one who made no hypocritical pretense of not wanting office but frankly asked for it, offering himself as an alternative to Hoover. Above all, he was apparently not frightened by the country's plight, but regarded it as a challenge to American ingenuity. He and the people together would find ways to overcome the crisis.

Hoover's failures, it must be admitted, were more of an asset than Roosevelt's promises. The judgment that the Hoover administration was a disaster deepened every day. The transfer of his presidential overlordship, on which Americans rely so heavily, was clearly being prepared.

The false trumpeting of an imminent recovery, when it was exposed, further undermined public confidence and even the most convinced conservatives were skeptical. For Hoover, it must have been a terrible disappointment that the signs of renewed activity were so soon shown to be false. They did soon begin to fade—had all but vanished by September—and, although they were persistently gilded by a slanted press, hardly anyone found Hoover's protestations credible. Conditions were, in fact, worsening. Decay was reaching farther and farther into the foundations of industrial-

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ism; doubts and disillusions about the system itself were circulating from person to person, and more and more openly. The whole nation seemed to be sinking into a bog of despair.

To make matters worse, other unnerving events took place, and all had the same effect. Each added its share to the weight bearing down on the chief executive. In the farm states, violence broke out repeatedly and on a larger and larger scale as prices fell to all-time lows. Then, in spite of Hoover's favorite rescue operation, the Reconstruction Finance Administration, designed to salvage financial institutions with frozen assets, banks continued to fail with spectacular repercussions. The RFC would be more effective later, but during these months results were not apparent. Besides, his concern with the corporations had led him to minimize the need for relief funds. He was confirmed in people's minds as one who relied on the "trickle theory." During the last weeks before the election this became a sour joke even among the least informed voters. He was sunk in a disastrous morass, pitiful to see.

In New York, the investigation of Mayor Walker dragged on, and no one knew then that it would presently turn into something of a triumph for Roosevelt, made complete by Walker's flight to Europe. The Republicans hoped for disaster, but they soon saw signs that it might be averted. The noisy controversy between Hoover and the Congress was ended with adjournment in July. Members went home unhappy, and even the Republicans found it discreet not to defend the president when they started their own campaigns.

There were other such happenings during those tragic weeks. None, though, was so dramatic and none polarized opinion so effectively as a most unlikely, almost trivial event—the treatment in Washington of the Bonus Expeditionary Force. Against this gloomy background the Bonus March—otherwise not so sensational—developed into a nationwide spectacle. It became a test of the courage and competence of those in authority—and especially, of course, of Hoover. He failed, miserably. People suddenly saw their president as a frightened and hapless executive resorting to violent reaction rather than the benevolent tolerance so obviously called for. Things had clearly become more than he could handle. He could no longer be relied upon to keep his head, and this was no time for bad judgment. It was his final failure—the symbolic end.

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This outcome was the stranger because Hoover's calling out of the military and the unnecessarily savage repression of the marchers, under other circumstances, would almost certainly have brought him extravagant acclaim. Such actions had done so for other presidents in other times. When Cleveland in 1895 had sent soldiers to suppress Pullman strikers, beginning a small civil war on Chicago's south side streets, he had been warmly and widely praised as the protector of law and order. It had been one of the few occasions during that president's second term when any considerable public approval had come his way.

In 1932, however, some evil magic seemed to be at work. Even actions that citizens might have been expected to hail as saving the nation from revolution brought Hoover only dispraise. There were early exaggerations of the threat to public order, which dwindled into reluctant apologetics. It had been like using a sledgehammer to kill a fly, perfect material for such flamboyant orators as Fiorello LaGuardia, Robert LaFollette, and others who, as sentiment changed, commanded wide attention. Americans were uneasy, but they were not ready to concede imminent overthrow of the government by a few thousand indigents who, not long ago, had been its defenders. People might be having it hard; but they were not persuaded that their veterans were about to storm the White House or the Capitol.

There were, however, mutterings from both Right and Left that tougher treatment was called for. The rightists had taken to praising Mussolini for making the notoriously inefficient Italian railways meet their schedules, and leftists were pointing out that there was no unemployment in the Soviet Union. Fascist and Communist alike offered order and security to those who were being made to bear the risks of a free economy. Mention of the accompanying regimentation was omitted, usually.

What was so novel was that anxieties were now spreading to a surprised and resentful managerial elite, who were beginning to reject the daily doses of optimism administered by the press and were growing more and more apprehensive; the unrelieved anxieties of the unemployed were obviously justified. Their hopes did not notably fasten on any foreign ideology, and they were hopelessly unorganized, so the fears on the part of their betters were actually hollow. The real victims were strangely long suffering even when their calamities were unbearable. True, the farmers were objecting

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loudly, but their demands were for a more equitable share—not for revolutionary changes. Even leaders of labor only sporadically voiced any indignation.

It was impossible to say how far disillusion with the administration had spread, but one in Roosevelt's position was bound to hear as much as anyone about the evidences. Tycoons and labor leaders, social workers and philanthropists, clergymen and press proprietors, farm organizers and officeholders (by the dozens) crowded the Albany house and his anterooms in the capitol. The advice they had to offer was sufficiently high keyed and alarmist, but it continued to be unfocused and diffuse. There was no agreement about remedies and, actually, no deeper disaffection than the call for Hoover's defeat. More than anything, as Roosevelt said wonderingly, there was a desire for leadership. People, even those who held themselves out to be popular tribunes, wanted reassurance—an authority they could rely on.

The Bonus March, when it began to find its way into the newspapers of cities along the route of its progress toward Washington from the northwest, inevitably recalled the similar march organized by Jacob S. Coxey of Ohio in 1894. That descent on the seat of federal power had petered out on the lawns of Capitol Hill, and Coxey himself had been jailed as a vagrant. The early fears it had aroused were soon dissipated, and even the hoots and jeers that had betrayed the relief good householders felt soon disappeared from the front pages. There was an obvious hope now that the Bonus March would have a similar end.

The circumstances, however, were different. W. W. Waters, the Coxey of 1932, had veterans of more recent vintage to count on. They had been in the army no more than fourteen years ago. They were in their thirties, men who ought to have been in their busiest and most productive years. Their families were no more than half-grown, perhaps not even complete. Their inability to find any sort of work was a grievance made more acute by new conceptions of governmental responsibility and by recent years of rising prosperity. Above all, what they were asking for could be made to seem a just demand. They were not demanding the consent of the Congress to a general easy money policy as Coxey had been; these veterans were asking only for the fulfillment of what they regarded as an obligation. In 1925, Congress had "adjusted" the compensation for

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their military service and had authorized certificates payable in 1945. They were petitioning for immediate payment of an acknowledged but postponed debt; it was hard to picture this as an outrageous demand.

Like many others who watched the papers, we had first begun to be aware of the marchers when they encountered trouble in East St. Louis on May 21. There were not many of them—a few hundred. They had been making their way east by boarding freight trains after the fashion of the Depression's indigent thousands. They called it "riding the rods." They camped along rights of way and climbed into the empty box cars of slow-moving trains. In good weather, passing freights had riders on roofs and others sitting in the doorways with their legs hanging out. This was against railroad rules and even against protective laws, but train crews quite generally overlooked the rules. They too might have to take to the road; no one had much security.

At East St. Louis, however, railroad officials had a sudden seizure of indignation and a guard was set up to prevent illegal boarding of eastbound trains. Since the suggestion was rumored to have come from Washington, it seemed to the marchers a provocative cancellation of their unspoken arrangement with authorities, and the professions to pacifism of the less stable marchers broke down. Gangs got together and soaped rails, uncoupled cars, and generally made nuisances of themselves. This brought in the police and enough trouble resulted to become national news.

It must be understood that all over the country there existed a hushed expectation that something dreadful might come of all the cutting of ties with places of work and settled homes. Even the most secure citizens could understand the humiliations of indigence and of hopelessness that had no promise of an end, and they were frightened by its implications. The signs at East St. Louis were small ones, but they were ones among many, and they were peculiarly sinister because these were men cut loose from traditional ties, and they were more or less organized. The veterans' initiative might spread.

The Baltimore and Ohio Railway guards were successful enough in repelling boarders so that the municipal authorities had to do something about their unwanted tourists. They resorted to traditional methods. Marchers were crowded into trucks and sent out

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of town—eastward. After a few miles they were unloaded, and jungle camps outside other towns were suddenly a problem. The publicity about the East St. Louis incidents ensured a close watch as the marchers made their way toward Washington. Officials prepared to meet the invasion much as East St. Louis had done, but now the problem was more formidable. Other unemployed veterans suddenly felt that the country they had fought for owed them something better than they had been getting. From many other towns on many other railways, volunteers gathered and started in the same direction. It became a movement.

The Waters contingent from Portland, with its accretions, amounted to about one thousand when it finally reached Washington; but on almost every arriving freight, coming from all directions, a dozen or more joiners came to the railway yards looking for their comrades. Very soon there were two thousand, then five thousand, and by July there were something like eleven thousand. By then, too, a large slum-like community was well established on the flats beside the Anacostia River and some sort of order had been achieved. When there was no more room, other sites were found and simply squatted on. With no water supply, only primitive sanitation, and the Potomac Valley's summer sun beating down, the collection of tents and shacks, almost under the noses of congressmen, were Hoovervilles they could not ignore.

What amenities there were in these camps resulted from the sympathetic efforts of the decent and kindhearted man who chanced to be superintendent of police in the District, Pelham Glassford, who had been the youngest and one of the ablest brigadier generals in the Expeditionary Force of 1918. He chose to regard the campers not as tramps, but as old soldiers fallen on hard times. Easy and humorous, he rode among them on his big blue motorcycle, gave them advice, and got for them what supplies he could from all the sources he could think of.

He was regarded as a friend and protector. One was needed, because their growing numbers were regarded as a menace more and more by householders in their vicinity. Anacostia and other small "cantonments" became more crowded, the summer heated up, and the effluvia from garbage heaps and latrines became almost unbearable. Arrivals increased daily and moved into any empty lot or abandoned building they could find. Commander Waters said there

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were nineteen of these and the population had grown to 23,000. Glassford thought 11,000 nearer the fact, but even that many, having soon lost any reason for pleasing the Congress by being orderly and submissive, grew harder and harder to control. The District commissioners were disinclined to support Glassford's tolerance and finally they simply ordered him to disperse the visitors by a date in the near future. The whole country was watching. It was an intolerable situation; however, it also seemed insoluble.

Only two among the authorities knew what to do about it. They needed permission, of course, and finally after an incident or two—that, it afterward appeared, were provoked—they got it. The two were Secretary of War Patrick Hurley and Chief of Staff Douglas MacArthur.

While fears were rising and excited publicity conditioning the public to demand drastic action, the Congress was considering the bill authorizing immediate payment of the bonus certificates. In June, it had passed the House; but in July, it was rejected by the Senate. Members of the Congress then went home and left the problem to others. The marchers no longer had the same excuse for staying in Washington, but most of them had no reason for leaving either and now they had a grievance. Besides, their number went on increasing. The momentum would be hard to check.

Glassford thought something more humane was called for than seemed to be contemplated by federal authorities and the District commissioners, and he conceived the idea of reconditioning an abandoned army camp not far from the city where some sort of life could be organized for those who were homeless. Before he could make much progress, however, the inevitable infiltration of real radicals began, and when John Pace of Detroit, a notorious agitator, persuaded some two hundred like-minded fellows to gather outside the White House, Washingtonians became as hysterical as Chicagoans had been in 1895 when Cleveland and Olney had chosen to regard railway strikers as rebels.

Armed guards were posted to watch the White House and hasty high-level conferences were held. The imaginary threat to the government became front-page news. Within a few days the whole country was watching the president. What would he do?

Hoover, harassed by advice from disoriented assistants and impatient generals, could think of nothing better than suppression.

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And on the morning of July 28, MacArthur with Major Dwight D. Eisenhower and Captain George Patton in attendance personally directed the deployment of cavalry, infantry, and tanks on Pennsylvania Avenue. The troops systematically routed squatters from the buildings they had been occupying along the avenue, then marched in column up Capitol Hill and went on toward Anacostia. There, they unslung rifles, flung tear gas bombs, and seized and carted away all those who resisted. That done, the veterans and their families—such as had come along—had to watch the soldiers raze and burn the poor shacks and tents that had been used for shelter.

For days the roads leading into Maryland were crowded with refugees, herded and harried by Maryland police. Thousands dispersed into Washington streets in spite of blockades and made their way west out of the city. The mayor of Johnstown, Pennsylvania, a veteran himself, had watched the proceedings with growing indignation; he now offered a refuge in one of his city parks, an offer vetoed at once by a fearful city council. Since this was the only suggested haven for the refugees, they had nothing to do but trudge along the highways, begging for rides, many quite literally starving. No one took pity on them, and they were soon forgotten in the press of other problems and were somehow absorbed into the miserable bands of refugees so prevalent in the countryside.

The week of these happenings, the last in July, had been one of pressures and problems in Albany too. What we knew of the Battle of Washington was what we read in the *Times*. Such warmhearted spectators as LaGuardia, LaFollette, Wagner, and many others, outraged but impotent, told us in detail what had happened. I discovered that, in spite of other matters that would not wait, Roosevelt had been told too, or had read the *Times*, and had drawn his own conclusions. On July 29, the expulsion occupied many columns, and there was a whole page of photographs.

On that same day, there was a report of Mayor Walker's reply to the Seabury charges, something that demanded immediate study. It would be on Roosevelt's desk when he got there. Yet about 7:30 a.m., when Roosevelt called from his bedroom and I went in, the page of pictures was open on his bed. They looked, he said, like scenes from a nightmare. He pointed to soldiers stamping through smoking debris, hauling resisters, still weeping from tear gas,

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through the wreckage to police wagons; women and children, incredibly disheveled and weary, waiting for some sort of rescue. In the lower right hand corner, the victorious general, one of the few in our whole history to have led armed troops against civilians, stood nonchalantly by a fence, immaculate and smiling among the vagrants, drinking from a paper cup.

Roosevelt, spreading a hand over the picture page, reminded me that we had been going to talk more about Hoover, but so far as he was concerned, there was no need now. He ought to apologize for having suggested him as candidate in 1920, but Hoover had seemed then just what was needed. No one in sight had had such a reputation as he. He had managed a massive rescue operation. He had shamed generals and lazy officials; he had done what no one thought could be done. He was the world's most admired humanitarian. What hadn't been realized was that the man was actually a sort of timid boy scout leader. Since he had done so much for people and on so immense a scale, he was supposed to be a kind of superman.

Roosevelt went on to recall that the whole country had still admired Hoover in 1928; what he had accomplished in the Commerce Department had been notable. By then he had become the Great Engineer instead of the Great Philanthropist, and that was just what the country needed. He was the strong and honest man in the Harding and Coolidge cabinets. The contrast with the others in-those administrations—except for Hughes in State—was obvious to everyone.

Even when the Depression had come along, it had seemed something Hoover could handle better than anyone else. Economic management was his specialty. No one foresaw the disaster it would turn out to be. But from the Crash in 1929 to yesterday, when he had set MacArthur on those harmless vets, he had depended altogether on his business friends. He still talked about principles and he organized charities, but actually he hadn't done anything that was nearly big enough. Now, said Roosevelt, look where he was. He had surrounded himself with guards to keep away the *revolutionaries*. There was nothing left inside the man but jelly; maybe there never had been anything else.

Roosevelt went on to say that he himself had a smaller political problem than he would have believed before this incident had hap-

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pened. MacArthur and the army had done a good job of displaying Hoover to the country as a timid leader; he could not be reelected now. If the Battle of Anacostia had happened a little earlier it might even have prevented his renomination.

I made some objection. There were all those registered Republicans; they were pretty traditional. Wouldn't they vote the party ticket anyway?

Roosevelt said no; this went deep. They wouldn't like to vote for a Democrat, but they would like even less to vote for Hoover now. And his own name was Roosevelt. It was still almost as much a Republican name as a Democratic one, and it stood for energy, for action. He pointed at the pictures in the *Times*, reminding me, as he had done before—pridefully—that during his four years as governor in a state troubled by Depression, he had never called out the National Guard. The fat cats had wanted him to several times when they were frightened, but he never had. His answer had always been that suppression would not be effective when there were real grievances. It would only make matters worse. They had gone on suggesting that the country was in danger of revolution and that discipline was needed, but he had refused.

I asked what he actually had said to those who urged him to act. He grinned for the first time in that talk. He usually let them go on, he said; if he argued too much with them, they'd go away and say he was a softie, a coddler of radicals. A good many suspected that anyway. But now that Hoover had been frightened by a few poor unorganized fellows and the reaction had been against him, there was no need to say any more than had been said by the reporters.

I guessed he was right. There was a Hooverville bigger than the one in Anacostia down by the Hudson River just below Morningside Heights. The people there lived on scraps in shacks built of bits and pieces. Their clothes were rags, but they were too beaten to be dangerous except to health. The difference in Washington was that Pace had persuaded some of them to march around the White House. Wouldn't people sympathize with the president? Wouldn't they consider him justified in using force?

Not in these times, Roosevelt said. What Hoover should have done was to have talked with Waters when he asked for an interview. Then when those two hundred marched up to the gate, he should have sent out coffee and sandwiches and asked a delegation

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in. Instead he had let Hurley and MacArthur do their stuff. He might even feel sorry for Hoover, he said, if he didn't feel sorrier for those people—eleven thousand of them, the paper said. They must be camping right now alongside the roads out of Washington. They must be sleeping in the cold. And at least some of them had families. It was really a wonder that there hadn't been more resentment, more radicalism, when people were treated that way. He smoked. Then he said that, anyway, they would make a theme for the campaign.

He wasn't interested any more, he went on, in analyzing Hoover. Hoover had been turned inside out. Everyone could see what was there, and they wouldn't like it. Neither did he; he wouldn't feel sorry for him even in November.

If Roosevelt had had any doubt before about the outcome, I am certain he had none after reading the *Times* that day. But his last word was something to the effect that either Hoover had been very different in the war years or he himself hadn't known Hoover as well as he thought. He was just a bit wistful.

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Park Avenue Diplomacy—Ending the Berlin Blockade*

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The city of Berlin lies 110 miles within the Soviet-dominated area of the German Democratic Republic. Cut off from the rest of the city by the notorious wall, West Berlin is virtually an island.

The Soviet Union has tried repeatedly to make the position of the Western powers in Berlin untenable. The four-power talks on an assured future position for Berlin which began in March 1970 finally ended in agreement but details, left to be settled by the East Germans with Chancellor Willy Brandt's government, at this writing still await final adjustment. Looking back, one recalls that 1961 was a year of high tension as the Soviets harassed access to Berlin. So was 1959. Before that, in 1948, the most famous of the Berlin blockades was established by the Russians. The West countered with the airlift and took the case to the United Nations Security Council. As expected, the Soviet representative, Vishinsky, vetoed a resolution proposed by the six "neutral" members of the Council and the Western powers cooperated with a United Nations committee of economic experts who sought a formula to solve the question which Moscow claimed was the reason for their blockade,

* This article is based upon a manuscript which I wrote while a Senior Research Visiting Fellow at the Council on Foreign Relations.

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namely, whether the more valuable "West" mark should continue to circulate in Berlin to the alleged detriment of the depreciating "East" mark.[1] The UN experts finally reported to the Security Council in February 1949, but the Council never debated their report because other events intervened.

On January 31, 1949, Marshal Stalin chose to reply to a question which had been posed by Kingsbury Smith, European manager of the International News Service. It was the custom for journalists to file questions, which Stalin took up from time to time as the spirit moved him. Kingsbury Smith had asked whether the Soviet Union would be prepared to remove the restrictions on access to Berlin if the Western powers agreed to postpone the establishment of a separate Western German state pending a meeting of the Council of Foreign Ministers (the CFM). Stalin said Soviet restrictions could be removed if Western restrictions were simultaneously removed. He made no mention of the deeply embedded currency question.

I think it was Charles Bohlen, one of the two foremost U.S. Russian experts, who first called attention to Stalin's failure to mention the currency issue. Dean Acheson describes how the point was discussed quietly with a few top State Department officers—probably Bohlen, George Kennan, then director of the policy planning staff, Dean Rusk, and John Hickerson.[2] Acheson explained the matter to President Truman, who approved the procedures recommended. The first step was to insinuate in the secretary of state's normal press conference on February 2 a subtle "signal" to Moscow that the U.S. had noted Stalin's statement and that if the Russians were serious, a less public channel must be used for carrying the discussion forward. This step was intended to avoid giving the Russians the impression that the U.S. was leaping to the bait, for fear that the Russians might back away. The second step was to open a quiet channel. I was then deputy chief of the United States mission to the United Nations, spending most of my time in New York. I was summoned to Washington and discussed the situation with the secretary and other Department officers involved in the plan-

[1] The detailed account of the blockade and of the debates in the United Nations are set out in my article, "The Berlin Blockade and the Use of the United Nations," *Foreign Affairs*, L (1971), 163. In writing that article and this one, I have relied chiefly on records in the Department of State which have been made available to me.

[2] Dean Acheson, *Present at the Creation* (New York, 1969), 267.

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ning. All of us naturally thought of the United Nations because the case of the Berlin blockade had been swirling or floating through the channels of that organization for what seemed an interminable period. We were at home in the formal debates of the Security Council but equally adept in the use of the diplomacy of the *couloirs*. It was decided that I should find an opportunity to put a casual question to Jacob Malik, the permanent representative of the Soviet Union to the United Nations.

The opportunity came on February 15 when the Security Council debated the Republic of Korea's application for UN membership. Senator Austin was sitting for the United States that day, but I was assisting him since I had represented the United States in the Little Assembly when the United Nations Temporary Commission on Korea was persuaded to proceed with elections in the south despite its exclusion by the Russians from the northern part of the country. The United Nations was then still meeting at Lake Success in the hastily remodeled Sperry plant. The delegates' lounge was across a little hallway from the Security Council chamber. Delegates usually came to the lounge before a meeting began. I loitered near the entrance until Malik with his usual group arrived. Contrary to later public accounts, we did not meet in the men's room or over a glass of orange juice. In the normal way, I greeted him with a remark about the weather, a subject on which we could usually agree. One or two other matters were mentioned before I said, by the way, that I had noted in Marshal Stalin's recent statement about the situation in Berlin and the possibility of lifting the blockade, that he had made no reference to the currency question, and that I wondered if the omission were accidental. Malik said coldly that he had no information on the subject. Well, I said, if he should get any information perhaps he would let me know since I would be interested. With the customary handshake, we separated and went into the Security Council.

I made the usual memorandum of my conversation with Malik, classified it "Secret" and marked it for distribution to Senator Austin and four other members of his New York staff and to Rusk, Bohlen, and Sandifer, the acting director of the Office of United Nations Affairs in the State Department. I was promptly and properly scolded by Bohlen for my indiscretion; the memorandum was reclassified "Top Secret" and all copies distributed to members of

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the mission in New York were called in. Distributions of later memorandums were carefully restricted. We did not know whether the démarche would be productive but we knew that any leak to the press or any other publicity would effectively halt it. We felt there was some basis for the supposition that the Russians wanted to end the blockade. The airlift was a daily triumph for the West, and the counterblockade was pinching the East European economies. The Soviets had failed to push us out of Berlin, and the U.S. was moving forward steadily with plans for the establishment of a West German government.

On March 2, 1949, I was confirmed by the Senate as United States ambassador at large. My base shifted to Washington but I still dealt with some United Nations matters in New York. On March 14, the United States mission to the UN in New York received a telephone message from the Soviet delegation saying that Ambassador Malik would like to see me the next time I was in New York. I sent word that the following day, March 15, would be a convenient time for me.

The Soviet mission then occupied the handsome house on the corner of 68th Street and Park Avenue, the former Pyne residence, just across the street from the Council on Foreign Relations. I was led up to Malik's office on the second floor and greeted cordially. He inquired about my bandaged hand and I explained it had been burned in putting out a flaming pan on the stove in our kitchen. Malik countered with the information that he had been in bed for a week with lumbago, which he first contracted in Tokyo; there an American nurse had treated him and jabbed him with a lot of injections which made him yell—the first time, he said, he had been in the hands of an American woman. I asked whether it was the last time but he laughed that off.

We sat on opposite sides of a small table with the Russian interpreter between us. Malik's English is excellent, but he spoke in Russian, partly reading from a prepared statement. The fact that his interpreter took down all that was said made it the more natural for me to scribble down both Malik's remarks and my own.

Malik said that he had received a message from Moscow in response to his reporting the question I had put to him; Moscow informed him that Marshal Stalin's omission of any reference to the currency question was "not accidental." Moscow said that the cur-

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rency question could be discussed at the meeting of the foreign ministers when Berlin and the whole German question were discussed. I inquired whether this meant that Moscow attributed more or less importance to the currency question; Malik said the statement had no bearing on either the importance or the unimportance of that question. I asked whether Moscow meant that the blockade would continue during a meeting of the foreign ministers. Malik said that I had not previously asked that question and he had no instructions about it. He said if I had other questions we could have further discussions later. I said that I was not suggesting that we have any negotiations but that I would be glad to learn any more information he received. He asked what other questions interested me and I referred to Vishinsky's new post as foreign minister and wondered if he would be coming to the special session of the General Assembly which was to be held in New York in April. Malik hoped so since he himself was very busy with the Security Council; he regretted that I had left New York since there was more fire in the Council when I was there, which led me to ask him if he liked to play with fire. Amid this badinage, Malik asked if I had any views about other points made by Marshal Stalin in his statement of January 31. I asked if he were referring to the question of the so called establishment of a West German government. Malik said that was one of the points. I remarked that it was not necessary to urge the postponement of those steps as a precondition to a meeting of the CFM if one were held soon, since no West German government was actually in existence. As will be seen, this was a point on which later difficulties arose. We talked about reciprocity in the lifting of restrictions on access to Berlin; Malik said the Soviet Union's measures were adopted in response to those of the West and I replied that reciprocally I could make that same speech to him. I said I came to New York from time to time and would always be glad to talk with him if he received further information.

When I reported to the small group in the State Department, we agreed that here were opportunities to be exploited and dangers to be avoided. If we immediately told the British and French, the danger of leaks was increased. But if Moscow decided to exaggerate the talks as proof that the West had surrendered and that the United States was negotiating for a settlement of the Berlin question, we would be in deep trouble with our Allies just at the time when the

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final negotiation of the NATO treaty was being completed—the pact was to be signed in Washington on April 4. It was decided that I should tell the British and French UN representatives in New York, which I did on Monday, March 21, the same day that I had a second talk with Malik. Jean Chauvel attended for France and in Sir Alexander Cadogan's absence, Sir Terrence Shone attended for Great Britain; he wanted to know whether my first casual talk with Malik was on instructions from the State Department. I assured him there had been no formal plan to open negotiations but that "some of us" in the Department had talked it over and decided it would be worthwhile to drop a casual inquiry and see what the result would be. I later drafted a telegram from Acheson to Ambassador Douglas in London giving the gist of the Malik talks and warning about leaks to the press. I also telephoned the mission in New York to convey the same warning.

When I talked to Malik on March 21, he had a different interpreter who could not always find the right words in English so that several times Malik himself repeated to me in English what he had said in Russian. I observed that the interpreter had before him a carbon copy of a paper from which Malik read but he had not prepared a translation in advance.

Of course "some of us" in the Department had considered very carefully what I might say to Malik in this second conversation, but since we could not tell in advance what direction the talk might take, I had only guidelines to follow.

Malik said that he had told Vishinsky about our previous talk and that Vishinsky had said that if there should be an affirmative agreement on a date for a meeting of the CFM, there could be a reciprocal lifting of restrictions on traffic and trade to Berlin. I pressed him to state whether the lifting of the blockade would precede the meeting; for example, if there were a meeting on April 15, would the blockade be lifted on April 10, April 5, or April 1? Malik said the date for lifting the blockade could be prior to the date of the meeting. He asked how soon such a meeting could be held since Vishinsky believed it should be in the near future. I said I had no instructions on that point but would let him know; we agreed the arrangements would take some time. I said that I assumed there might be several matters to be discussed at the CFM and did he think these could be handled through ordinary chan-

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nels or through a continuation of our talks. I noted that there were two other governments which would be involved. Malik thought we might have some further conversation before putting the matter into regular channels. Referring to his notes, he said the currency issue was still considered important but it could also be discussed at the CFM.

We then turned to what was for the Russians the key issue, one on which Vishinsky was intent on getting a commitment. Malik said that Vishinsky attributed importance to my statement at our previous meeting that no West German government was now in existence. At this point there was trouble with the interpretation, but the indication was that I had said that we would "call off" the formation of a West German government if there were a CFM. I interrupted to remind Malik that I had made no such promise; I had pointed out merely that since no West German government existed at the moment, if there were a CFM now, it would meet in the absence of such a government. Malik confirmed my recollection and corrected the interpreter. He did not ask for any promise from me on this point.

Malik went on to say that Vishinsky saw no objection to continuing these informal talks with me and wondered what topics I had in mind. I said I had nothing particular in mind but just as a personal question would ask what he thought of discussing Greece. Malik doubted that that was an acute problem between our governments. He said the United States was involved through the presence of the U.S. military mission in Greece but the Soviet Union was not involved. I suggested that the Soviet Union had influence with the neighboring states. With a laugh, Malik said the USSR had influence with *some* of the neighboring states—Yugoslavia was not mentioned by name. Malik, when questioned, had no further subjects to discuss, but we continued to chat about the United Nations and the pending session of the General Assembly. Malik said he had seen a speech of mine in which I had said that the Soviet Union did not cooperate in the United Nations; he said this was not true, that the Soviet Union wished, as a matter of principle, to strengthen the UN. Somewhat petulantly he said the USSR never could get favorable action in the UN on anything it suggested. I replied it might get better results if it made better proposals. I asked why it did not cooperate in the Little Assembly. Malik said

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that Assembly was an illegitimate child, but admitted that Soviet law recognizes illegitimate children. I suggested there was therefore no reason why the Soviet Union should not play with an illegitimate child in the UN. In taking leave, I told him I would let him know when we might talk again.

There followed intensive talks in the State Department and a spate of memorandums. The special session of the General Assembly opened on April 5 and many officers were busy with preparations for those meetings; I was still one of the United States delegates. Bevin and Schuman were about to arrive for the signing of the NATO pact and arrangements for those meetings had to be made. The working group planning the Malik talks was now composed of Bohlen, Llewellyn Thompson, Robert Murphy, and, for part of the time, General Bedell Smith. At times Under-secretary Webb joined us. Bohlen continued to play the leading part and I had the advantage throughout of benefiting from his intuitive and reasoned analyses of the Soviet positions. Robert Murphy was appointed acting director of the Office of German and Austrian Affairs on March 4, 1949. He records that when he reached Washington he learned "that top-secret negotiations for ending the Berlin blockade had been going on for some time." Of course, there were no "negotiations" whatsoever, and the goings-on up to that point consisted of only my sounding on February 15. Murphy was a distinguished foreign service officer with long experience not only in Berlin but in all the dramatic events which he recounts with gusto in his *Diplomat Among Warriors*. His inaccuracies and omissions in connection with the Berlin blockade are rather surprising. He writes that "in 1949, when he was negotiating with Malik, Jessup was a professor of international law at Columbia University, on leave to serve with the American delegation to the United Nations." He may have resented my appointment as ambassador at large, a position which I held for almost four years. Murphy continues:

> When I talked with the Columbia professor, his attitude reminded me of Ambassador Winant's attitude in London five years earlier. Like Winant, Jessup felt that he had achieved an exceptionally cordial relationship with the Soviet diplomat, and he depended heavily upon Malik's good will to reach an agreement on Berlin. I did not succeed in convincing Jessup, any more than I had Winant, that Soviet negotiators

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cannot be influenced by personal friendships. At first Malik proposed outrageous conditions, making acceptance impossible. But after weeks of talk Malik receded from this extreme position, thus giving the impression of substantial Soviet concessions. The Jessup-Malik talks were kept so secret that I was not permitted to tell even Clay about them when I made a quick trip to Germany that spring. Clay was put in the embarrassing position of first hearing about the negotiations from his British colleague in Berlin.[3]

I never labored under the illusion that a Soviet diplomat would engage in free-wheeling diplomacy out of personal friendship for the representative of another country, but I did find in the course of some four years of official responsibility for handling negotiations, on the problems of Palestine and the Middle East for example, that friendly personal relations with both sides made it easier to negotiate. This fact of diplomatic life, especially in United Nations activities to which not all career diplomats have been exposed, makes it true that much important diplomatic work is done on social occasions, despite the ignorant blusterings of Congressman Rooney, who would prefer to have American ambassadors entertain their opposite numbers with ice water and pork and beans.

As for Malik's "outrageous conditions," I think we regarded them as normal opening gambits, utterly unacceptable, but quite in line with the Soviet position previously stated by Stalin and Molotov. And, actually, it was Murphy's friend, Vishinsky, rather than Malik who tried out various demands to see if they could block the establishment of the West German government.

On March 23, the working group considered at length the procedures for further talks with Malik. One question was the choice between reading a statement to Malik and leaving a paper with him. I opposed the first alternative because of the possibility of an inaccurate translation, but the other view prevailed. It was decided that it would be possible to mention a probable date for the establishment of a West German government without making any promise in regard to it. It was also agreed that final arrangements had best be made with Malik in New York by Cadogan, Chauvel, and me. Bohlen thought it important to bring the Security Council into the act in some way, and we discussed asking the president of the

[3] Robert Murphy, *Diplomat Among Warriors* (Garden City, N.Y., 1964), 320-21.

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Council to announce the final agreement as if it were the result of his good offices. These points I put into a draft of a memorandum for Secretary Acheson to use in his talks with Bevin and Schuman. In particular we wanted to know whether they would prefer to have me talk again privately with Malik or whether at this point they wanted to broaden the talks to include Chauvel and Cadogan. The memorandum indicated that we agreed with Ambassador Bedell Smith that lifting the blockade would be such a victory for the West that we could agree to procedural details suggested by the Russians. It was hoped that the three ministers would record their agreement that a meeting of the CFM would not interfere with or interrupt the continuance of the plans for carrying out the London decisions on the establishment of the West German government. I recalled that the French had cited the continuation of Security Council proceedings as an excuse for withholding agreement on the introduction of the West mark *B*. Bohlen had suggested the idea of pinning down the British and French by an agreed statement and we kept trying to achieve this. It was considered desirable to reach solid decisions before the news got out and Herbert Evatt of Australia, the president of the General Assembly, and Trygve Lie tried to get back into the act.

Murphy sent me extended comments on my draft. He did not believe that the French were stalling the talks then going on in London about the future of Western Germany; they were committed to the establishment of a West German government. He thought my statement to Malik should point out that the formerly alleged technical transport difficulties of access to Berlin were now nonexistent and I should stress the mutual benefits of East-West German trade. Although my draft pointed out that a CFM could not bring about a general agreement on the German problem, Murphy stated this as a new thought and said (as had long been agreed) that we should consent to holding a CFM only if the blockade were lifted as a condition precedent.

My memorandum, as revised and presented to the secretary of state on March 28, was also cleared by George Kennan. It suggested that I ask the Russians to agree that the president of the Security Council should suggest to the four powers the reciprocal lifting of the blockade and an ensuing meeting of the CFM; the four would

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then notify him that they accepted. However, we would not press this procedure if the Russians objected. The date for the CFM should be set so as to fit the need of Secretary Acheson to testify before the Senate Foreign Relations Committee on the NATO treaty and to be back in Washington before the end of the Senate debate. Looking ahead, it was suggested that there be an agreement to modify the formula adopted at Potsdam regarding meetings of the CFM so as to make it possible in future to have the Council composed of personal representatives of heads of state instead of foreign ministers.[4] Finally the memorandum suggested that the secretary ask the president not to tell the military about the Malik talks until after the conversations with Bevin and Schuman and the next talk with Malik. On March 29, President Truman approved the suggested procedures and the text of the statement to be read to Malik.

The secretary met first with Bevin on March 31, then separately with Schuman on April 1 and later that day with the two foreign ministers together. The British and French ambassadors accompanied their ministers, and Murphy, Beam, and I went with Dean Acheson. Bevin's reactions to the situation were cautious and worried, more so than Schuman's. Bevin feared that the French (and perhaps the United States) would hold up ratification of the NATO treaty pending an appraisal of further Soviet actions. But he was reminded that the three governments were already on record as agreeing to a CFM if the blockade were lifted, quite irrespective of the Malik talks. It was agreed that I should talk again with Malik and read him a statement about the West German government, but Bevin wanted the statement reviewed by a drafting group after which he said he would need to submit it to his Cabinet for approval. He suggested that we tell Evatt to keep hands off; Acheson said such a warning would have the opposite effect. The idea of

[4] This was one of the several unsuccessful attempts to relieve the secretary from the burden of attending so many meetings overseas. The idea of substitutes at a CFM was discussed with the British and French later in Paris when the ministers met. The idea of substituting a "Personal Representative of the Head of State" was wholly unacceptable to the British and the French. As Bevin made clear, if the foreign minister did not attend, they could only send a deputy of the minister, not someone having authority from the prime minister or the king. As for the Soviets, although Vishinsky did not operate with the same authority as the other three ministers, it was doubtful if he would attend a meeting if the United States sent a substitute for the secretary of state.

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giving any role to the president of the Security Council was not approved because the presidency was then held by Fawzi Bey of Egypt, and no one would suppose that he was likely to originate a plan for settling the Berlin question.

On April 3, I went to the British embassy to meet the ambassador, Sir Oliver Franks, and Couve de Murville and Bérard from the French side to draft the exact text of the statement for Malik. We based our discussion on a draft prepared by Ambassador Franks. It was understood that I was to tell Malik that on this occasion I was authorized to speak for the three governments. That evening Bevin approved the text and Schuman asked for one modification. We met again at the British embassy the next morning and made a few changes, which Franks took upstairs to Bevin who again approved. Schuman's approval was reported by phone that evening, and on the following day I went to see Malik.

At our talk on April 5, again at the Soviet headquarters, Malik had a very good interpreter; we talked for an hour and forty minutes. I began by telling Malik that I was authorized to speak for the three governments and I read him the agreed statement. Malik asked for clarification of the relative dates for lifting the blockade and for the holding of the CFM; I explained that when we fixed the date for the meeting we would have to fix an earlier date for the end of the blockade.

Most of the conversation related to what was, for the Russians, the key question—would the Western powers delay or postpone the preparations for the establishment of a West German government? Malik gave a fair summary of what I had said at our last conversation, and I underscored the fact that the West was making no promise about postponement and that, as the statement I had just read explained, if the CFM were held in the near future, it would be held before the West German government was established. Malik then read from typed notes which he had in front of him, recalling Stalin's reply to Kingsbury Smith and what I had said at our last meeting. Malik also read from his notes a summary of the points Vishinsky had made and to which I had previously responded. The argument went back and forth; when I said that in mentioning "the reasonably near future," I took into account that it would probably take five or six weeks to complete the preparations for a CFM, Malik asked if that meant we would suspend the preparations

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for a West German government for five or six weeks. Again I had to explain that the steps necessary for the establishment of the West German government could not be completed in five or six weeks, but that we made no promise at all about that date and did not agree to any interruption or suspension of the preparations for establishing that government.

The conversation then shifted to a discussion of the agenda for a CFM. The agreed statement which I had read to him mentioned "matters arising out of the situation in Berlin and matters affecting Germany as a whole." After discussion, Malik agreed that this formula would include the currency question, which the Soviet government still considered important.

Malik inquired whether the United Kingdom and France had made any further suggestions about the date and the agenda. I replied that the statement which I had read to him was designed merely to make certain that the United States had clearly understood the proposals which Malik had made to me as the views of the Soviet government and to indicate that if the understanding was correctly registered, we might then proceed as a next step to fix an exact date and a detailed agenda. Malik asked what the next step would be, and I told him that if the Soviet government wished to proceed, future discussions would need to include representatives of France and Great Britain. Did that mean, asked Malik, that the two of us could have no further talks alone? I assured him that I would always be glad to talk informally with him but that if an agreement was to be formalized the others would have to be included. Why then, asked Malik, did they not make some suggestions about the date and the agenda? I said we had an open mind on those points. This stumped the interpreter and Malik said (quite innocently) that there was no equivalent for "open mind" in the Russian language, but he understood what I meant. We then agreed that our conversations were in no way binding and Malik said he would report to Vishinsky. I said I would await word about meeting him again.

I found opportunities to report directly on this talk with Malik to Bevin and Schuman as well as to their staffs. Schuman thought the results would have historical importance, and Bevin was pleased that I had stressed the fact that we would not cease to prepare for the establishment of the West German government. Curiously, it

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seemed impossible to secure an agreed minute of the three ministers on the same point. Couve de Murville told me that it was satisfactory to Schuman, and Franks said Bevin had no objection to the substance but was doubtful about recording it in a minute. As I reported to Bohlen, Kennan, Murphy, and Thompson, the ministers agreed that I should keep in touch with Cadogan and Chauvel in New York and that there would be talks among the representatives of the three governments in Paris, preparatory to the CFM.

On April 8, the three ministers issued a communiqué reporting that they had reached complete agreement on a whole range of issues connected with Germany; there was no mention of Berlin. Secretary Acheson also issued a statement to the press reporting the final success of many long negotiations among the three governments and hinting at the numerous arrangements still to be made before the West German government could be established.

Responding to a phone call from Malik, I went to see him again on Sunday afternoon, April 10. He read me a statement which said that Vishinsky had received the report of our last conversation. Vishinsky understood that I had stated that a West German government would not be established either before or during a meeting of the CFM. Since this was so and since the parties agreed on lifting simultaneously restrictions at Berlin, there was no obstacle to holding conversations regarding the date and the agenda, bearing in mind that the date would be within five or six weeks and that the four ministers would attend. The date for lifting the restrictions could also be arranged.

Naturally, I immediately played my side of the record and Malik agreed I had correctly recalled what I had previously stated, but he then reread Vishinsky's statement and I had to repeat mine. I told Malik that on this crucial point there should be no misunderstanding between us. Malik agreed and asked me to report Vishinsky's statement to Mr. Acheson. I pressed him to state the exact issue, but he would not put any gloss or interpretation on Vishinsky's statement which he would like to have conveyed to Mr. Acheson. I had the impression that Malik knew perfectly well what our position was but that Vishinsky still hoped, without breaking off the dialogue, to rephrase the "understanding" in such a way as to delay the establishment of the West German government.

On April 12, Secretary Acheson sent my report on the last talk

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with Malik to President Truman. He suggested that the president might now wish to inform the Defense Department as well as Generals Bradley and Clay, while Acheson informed our diplomatic representatives in Berlin and Moscow. The president was worried about the news leaking, and it was not until a week later that he informed Acheson that he had told Secretary of Defense Louis Johnson and authorized him to inform Clay through Bradley. Meanwhile Clay had been informed by his British colleague in Germany and was quite understandably annoyed.

Rumors were beginning to appear in the press on both sides of the Atlantic and several drafts of possible public statements were prepared. Couve de Murville told me that the French were worried lest the Germans now delay setting up of their government because the CFM was to meet. Kennan drafted a long memorandum describing the historical background and giving assurance to the Germans, to the people of Europe, and to Americans. Bohlen distributed a memorandum stressing the need to avoid damaging our German program but doubting whether the Russians would break the story of an agreed meeting of the CFM until the talks had definitely been concluded one way or the other, although they could embarrass us by a formal public offer at that juncture. The secretary drafted a possible statement to the press that the Department would neither confirm nor deny rumors about Soviet feelers on settling the Berlin crisis.

Dean Acheson made me responsible for coordinating preparations for a possible CFM, and preparatory work began in the Department. I asked George Kennan to talk with Murphy, who was asked to begin preparing background papers. I sent a memorandum to Bohlen, Kennan, Murphy, and Thompson which raised also the problem of the preparatory three-power talks before the CFM. There were meetings with the secretary of state, which Under-secretary of State James Webb and Dean Rusk also attended. There was not unanimity on our whole policy toward Germany. I told the secretary on April 19 that I thought we should agree with what Kennan had stated earlier: "We will not favor a united Germany in a divided Europe." A little later Webb sent me a letter from Averell Harriman advising that we fix our positions and not vary it one iota; that was the only way to deal with the Russians.

Meanwhile I had to spend some time at the General Assembly in

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New York, which gave me a chance again to use the casual meeting ground of the UN at Lake Success for talks with Cadogan and Chauvel, although we met also at Cadogan's home at night. I talked also with Couve de Murville and when I chanced on Malik I told him I hoped to see him the following week. "Any time," said Malik. As usual our Allies were not of the same opinion on all points. Confusion arose from discrepancies in reports by different diplomats of their conversations. At one moment Bevin wanted to hold up any further talk with Malik until the Bonn constitution had been approved. René Mayer told me at a luncheon that the French Communists would try to hold up the NATO treaty on the ground that the CFM meeting meant there was no danger of war. Bevin wanted the three of us to meet the next time with Malik, but Cadogan shared Chauvel's view that I should have one more solo talk. Schuman had telegraphed Chauvel that "Two is a dialogue; four is a conference." The ministers by now were in London and Paris and had to be reached by cable. Acheson cabled Ambassador Douglas at London most secretly to see Bevin at once and try to reach agreement, because Cadogan told Chauvel and me he could not agree to Chauvel's suggestion that I set an early date for my next meeting with Malik, although Cadogan thought Bevin's position was based on a misunderstanding.

Meanwhile there had been intensive work on the text of a statement I would read to Malik at our next meeting which I set for April 27. Tass, alleging that the West was circulating false rumors, published a reasonably factual statement on April 26 to the effect that the Soviet government was prepared to lift the blockade and to hold a meeting of the CFM. I had drafted a full statement which had been approved by the president and it was released by the State Department on the same date. It gave the history of my talks with Malik. The press release concluded with the statement that final agreement could not be reached until there had been further talks with Malik.

I had difficulty seeing Malik on April 27, not that he was reluctant, but the crowd of correspondents and photographers at the corner of Park Avenue and 68th Street was almost impenetrable. Once inside, I read Malik the statement which had been carefully prepared and agreed to by all concerned. It began by saying that Secretary Acheson had given careful consideration to what Malik

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had told me on April 10, including the views of Vishinsky. Before there could be any formal arrangement it was necessary that there should be clear understanding of what was agreed. Our standing position was then restated. Malik went over it with me point by point. He and I had different dates in mind about the time when the first restrictions were imposed; we agreed that was a matter of fact which could be cleared up. We agreed that it was intended that *all* restrictions should be removed. Our statement said that we should end the CFM meeting in the second week of June. (That was when Secretary Acheson had to be back in Washington for debates on NATO in the Senate). Malik asked when the CFM would begin; I said we could fix that date when we met with Chauvel and Cadogan if all the principal points were agreed upon. He then asked me to reread those passages in the statement which referred to the continuation of preparations for the establishment of the West German government. He repeated correctly after me the exact points involved. Was it understood that the CFM could consider all questions affecting Germany, including the question of the currency? Yes.

The next step, I said, was for us to meet with Cadogan and Chauvel. There might be a four-power note to the president of the Security Council telling him of the agreement. The Soviet government might send the three governments a note containing details of the agreement which the three would then confirm in the usual way. Malik thought that unnecessary; we could issue a four-power communiqué stating simply that the four had agreed to lift all restrictions and that a CFM was to be held on a certain date to consider the question of Germany including the currency question. He saw no need to inform the president of the Security Council. Later it was agreed that since the three governments had put the item on the agenda, they would inform the president of the Security Council that it could be removed when the restrictions were lifted. Since the French representative was president of the Council during May, notice of the agreement could be sent to the secretary-general. We discussed the length of the interval between the lifting of the blockade and the holding of the meeting; Malik was not prepared to discuss exact dates. He would report on our conversation to Vishinsky and say that in his opinion we had reached agreement. It would be up to Vishinsky to decide whether this was correct. He

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agreed that if Vishinsky approved his report, the next meeting should be with Cadogan and Chauvel but he had enjoyed our private conversations. I replied that I was generous enough to want to share the pleasure with my two friends, to which Malik replied, "I am an individualist."

We took note of the crowd of reporters outside and agreed not to issue any statement.

But the process seemed endless. I went to see Malik again on April 29 and found that he wished to settle final details about dates and other items with me then. I told him I had come merely to ascertain whether Vishinsky had approved the conclusion that Malik and I had reached agreement on the points we had covered two days before. He had the interpreter read slowly to me the draft of a communiqué which he had prepared and wanted me to agree on dates. I told him I would report his proposals and then we could meet with the others. He said he would like to have my views before the four of us met. We agreed to tell the press merely that we had had another satisfactory talk.

I reported promptly to Cadogan and Chauvel, but before Bevin had received Cadogan's telegram he had instructed Cadogan that the description of the restrictions to be removed must be worked out in detail, which could not be done in New York but only by the military governors in Berlin. We three recalled with a shudder the fate of the August 30 Directive when discussed by the military governors. Bevin said it was too soon to fix a date for the meeting of the CFM or for the lifting of the blockade. But Bevin sent via Oliver Franks a long personal message to Acheson in which he expressed appreciation for the way I had handled the talks with Malik.

In my meeting with Cadogan and Chauvel, the former asked to have inserted in the draft four-power communiqué, a paragraph which seemed to introduce a new element on which I wished to consult the Department. I telephoned Rusk, who talked the matter over with Kennan, Bohlen, Murphy, Byroade, Nitze, Reinstein, Trivers, and Beam. They said the paragraph must be omitted since in trying to specify the type of restrictions to be lifted, Bevin had unintentionally used words which might have affected the general trade policies which the United States had adopted.

We did agree in Washington and New York, however, on the

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text of a letter which I sent to Malik on May 2, in response to his request. I reported we were in agreement that the place of the CFM meeting would be Paris. He had proposed meeting between June 10 and June 14; we proposed May 23. He had proposed lifting the blockade one week before the CFM; we proposed a fixed date of May 9. To identify the date of the imposition of restrictions, he had suggested March 30; we suggested March 1. Then there were detailed alterations in the phrasing of the communiqué. I proposed that we four meet at the office of the U.S. mission to the United Nations at 2 Park Avenue the next day at 12:30.

Malik countered with the suggestion that we meet on May 4, which we did. Before he arrived, Cadogan, Chauvel, and I went over a few last points. We agreed that the three Western governments would tell the Security Council that the item of the Berlin question could be taken off the agenda as soon as the restrictions were removed on the agreed date.

The communiqué was duly issued on May 5. The restrictions were removed and the CFM met in Paris as scheduled.

The resort to the Security Council in September 1948 was a direct and exceptional use of the machinery of the UN, notable since it is commonplace to say that it was not ever expected that the UN could solve disputes between the great powers themselves. In my statement on behalf of the United States in the Security Council on October 4, 1948, I said: "The real question is whether in the present situation the only existing general international machinery for the preservation of peace can be used to remove a threat to the peace, or whether, as the government of the Soviet Union contends, the world must be thrown back upon the unorganized international community with all that implied." Granted that this statement was part of the public presentation of the case of the United States and was designed to influence public opinion, it really was a point of view held in the State Department during those first few years of the existence of the United Nations. The more sophisticated and cynical attitude was the product of later years when the United Nations, no longer a novelty, proved to be a disappointment to the general public because of its weaknesses.

The resort to the Security Council was a genuine use of the United Nations, but I would agree with the view, which has been stated by more than one writer, that discussions such as those with Am-

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bassador Malik might have taken place in Washington or Moscow between the accredited ambassador and an officer of the State Department or Foreign Office. Nonetheless, it is true that Lake Success and New York, like Geneva in the days of the League of Nations, offered and still offer superior opportunities for unpublicized contacts among representatives of various states and this is a value which is not negligible.

There was, however, another direct involvement of the United Nations in the attempts to settle issues arising from the German problem twenty years ago. Berlin did not suddenly become a haven of peace after the meeting of the CFM in the spring and summer of 1949. The four-power communiqué of June 20, 1949 did not actually include mention of currency but as Secretary Acheson stated to the press:

> arrangements have been made so that the Commandants can meet and discuss the thousand and one problems of the split city. No veto resides in anybody, but a forum again is created where people can meet and talk about the intensely irritating situation which would exist here in Washington if this city were split at Fourteenth Street and you had one government on one side with one currency and another government on the other side with another currency. . . .

The original institution of the Western mark was a brilliant success in its effect on the economic situation in West Berlin, which became a showcase of prosperity in contrast to the bleak conditions in the Soviet zone. The Soviet government naturally wanted to level this difference, but as the blockade was met by the West's counterblockade, the damage to the Eastern bloc's economy by this restriction on trade was probably a more important factor than the institution of the mark in persuading Moscow that the effort to impose its will on the West was a failure. Even more persuasive must have been the daily realization of the impressive impact on world opinion of the unprecedented success of the airlift.

On April 17, 1950, I sent a memorandum to George Perkins, assistant secretary for European affairs, and to Henry Byroade, director of the Office of German and Austrian Affairs, suggesting that with France and Great Britain we might well send a communication to the Security Council noting what had been agreed at the CFM, that the Berlin question was still on the Council's agenda and that lately there had been renewed though minor difficulties on

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access to the city. The communication, I suggested, might mention the official East German statements about the demonstrations which had been organized to force the West out of Berlin. Perhaps a renewal of Security Council consideration could lead to assurances that the Soviet Union had no intention of questioning the right of the Western powers in Berlin.

Nothing came of that proposal but while the General Assembly was meeting in Paris in the autumn of 1951, the Department telegraphed the text of a long statement which it proposed the secretary should make on the subject of German unity. The statement would recall that the four powers had several times urged free elections throughout Germany but that General Chuikov had not replied to any of these proposals. From Paris, Ambassador David Bruce advised waiting for the views of John McCloy, who had succeeded General Clay as high commissioner, and avoiding any statement on the subject before Chancellor Adenauer had been advised. If there were no objection from those quarters, it was agreed with the British and the French that by October 22, 1951, the three powers should send urgent identical notes to the secretary-general of the UN asking him to submit for inclusion on the agenda of the General Assembly a request from the three governments that the UN appoint a commission to examine the extent to which conditions throughout Germany would make it possible to hold free elections.

Anthony Eden, who was then British foreign secretary, suggests in his memoirs that this proposal for a UN commission to consider the question of free elections in Germany was connected with the overall problem of countering the Soviet drive to paralyze the negotiations for the final agreement on the establishment of the European Defense Community. In Eden's view, the Soviets realized that if Germany remained divided, they could not prevent the German Federal Republic from joining the Western defense organization, but they were afraid to end the division of Germany by genuinely free elections. His analysis is supported by the subsequent Russian proposals to discuss the unification of Germany in a four-power conference instead of establishing a UN commission on German elections.[5]

On October 29, Ambassador Bruce telegraphed to the Depart-

[5] Anthony Eden, *Full Circle: The Memoirs of Anthony Eden* (Boston, 1960), 49.

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ment the text of a letter to the secretary-general prepared by the French and calling for UN supervision of elections in the Federal Republic, in Berlin, and in the Soviet zone of Germany. The French would have no objection to stating that this proposal was made at the request of Chancellor Adenauer. But the next day, Paris reported that Schuman wanted to discuss the matter with Secretary Acheson; one of his staff had remarked that there were "too many lawyers mixed up in this on both sides." On November 3, it was reported that the British were eager to go ahead with the note lest the Russians seize the initiative with some move of their own; the text was to be finally perfected in Paris. On November 5, 1951, a note along the original lines was sent to the secretary-general, one proposing an investigation of whether conditions were such as to permit the holding of free elections with a view to the unification of Germany. On November 13, the Assembly voted 47-6-2 to place the item on the agenda and to refer it to the Ad Hoc Political Committee. The three governments introduced a resolution which Senator John Sherman Cooper of the United States delegation stated had as its object "to secure the aid of the United Nations in taking a necessary step toward the unification of Germany." The Soviet Union, of course, opposed the resolution, using the argument that Article 107 of the Charter denied the competence of the UN to deal with the German question. On December 20, the resolution was adopted by the General Assembly, 45-6-8, providing for a commission composed of representatives of Brazil, Iceland, the Netherlands, Pakistan, and Poland. By the resolution, the UN also declared its readiness to offer its assistance in guaranteeing freedom of elections. Poland refused to serve and the other four representatives proceeded to organize their work. The Western powers responded promptly to a request for assistance in carrying out the task, but the Soviet Union said nothing at all.

In the meantime, during the consideration by the General Assembly, there was the usual discussion with the British and French about parliamentary tactics. The U.S. delegation at first urged that both West and East Germans should be heard by the Assembly, but shifted to support Adenauer's opposition to such a development. The Department first had referred to its traditional stand in favor of hearing all parties, but told the delegation not to raise the question. If it was proposed by others, the United States should not vote

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against it. Actually Soviet Ambassador Malik had suggested to Ambassador Ernest Gross that the German groups should be heard. The question continued to be argued back and forth over the wires. The Department, however, warned against opening the whole German question. If the Soviets, or others, suggested a four-power meeting to discuss the German question, the United States delegation should point to the sorry history of repeated Soviet disruption of efforts along those lines, but the delegation should not flatly reject the idea. If the Soviets were sincere, it could be pointed out, they could give no better proof of their sincerity than to support the proposal for a commission to supervise elections. Shortly before the final vote, the Swedish delegation proposed as a substitute that the four powers supervise the elections. The State Department reacted vigorously, urging our delegation to persuade others not to accept the Swedish idea. Strong representations were made in Stockholm, and Ambassadors Bruce and Gifford were instructed to see Schuman and Eden and urge them to take similar steps. There were démarches also in Bonn, Oslo, and Copenhagen.

When the resolution creating the UN commission was adopted, Mayor Reuter suggested that the commission should come to West Berlin even if denied all access to the Soviet zone. The Pakistani member of the commission opposed this suggestion on the ground that in accepting the hospitality of the West, the commission would lose its impartiality; it should go to Geneva and report from there. The French and Dutch supported the Pakistani view as did the United Kingdom. The United States was content to leave the decision to the commission which, it was felt, would not agree to a one-sided effort.

By March 1952, it was clear there would be no Soviet cooperation with the commission, which spent a week in West Berlin and the Federal Republic; the United States then acquiesced in its going to Geneva to prepare its report. The commission intended to make one more appeal to General Chuikov and the Soviet high commissioner but no affirmative response was expected or received. The commission filed its final report on August 5, 1952, stating that it was unable to carry out its task because of the lack of Soviet cooperation. But it stated that it continued "to remain at the disposal of the United Nations" whenever it seemed possible to accomplish any results. The report followed the lines suggested by

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the State Department in a telegram of August 1 to the United States mission to the UN.

From the evidence at my disposal, I do not know whether the United States expected that this activity of the UN would produce any result. The experience of the similar UN election commission in Korea, which was denied access to the northern part of the country under Soviet control, must have suggested that the Soviet authorities would not have welcomed a United Nations commission traveling around Eastern Germany interrogating people about their freedom from any suppression of democratic liberties. On the other hand, the proposal contained no risks and might possibly have been met by a change in the Russian posture. It is likely that there was agreement with the British view which has been mentioned. Under such circumstances, when our French and British allies favored going to the UN, it is doubtful that any strong opposing voice would have been raised in the State Department. On the contrary, many officers would have favored it on principle.

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Soldiers in Politics: New Insights on Latin American Armed Forces

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In Latin America the 1960s was a period of widening military roles in politics and renewed concern with military behavior in political life. As the armed forces took a larger share in government, there arose a corresponding pressure for explanations and interpretations of military behavior. Historians, however, had barely started basic studies of military institutions in the national politics of individual countries and political scientists had only begun to roll up their guns. In the absence of much information about national military establishments, the studies undertaken in the 1960s had to be based on guesses or incomplete information in some important areas.

Under the pressure for quick results, the process of appraising and explaining military behavior was also subject to the pull of contrary forces because of the highly controversial nature of the subject. As a result, explanations and judgments of military behavior became influential when they had value as arguments either for or against the legitimacy of the military's roles in politics. Once a stereotype of military behavior was formed, gained currency, and became widely accepted, it gave rise to a countering stereotype in the continuing debate. Consequently the dominant theories used to explain, interpret, and judge military behavior in politics and to provide a basis for analysis and recommendations

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about how to deal with the Latin American armed forces developed in pairs. They took the form of matching propositions—approving or criticizing the military—about the motivation and effects of the military when it becomes involved in political action.

As it evolved in the 1960s, the image of the Latin American military was obviously the work of many hands and was also influenced by events and policies of the time. It mattered, moreover, that, under the pressure for explanations and interpretations of the political roles of the military in Latin America as a whole, scholars approached the task through areawide studies rather than by studies of how military institutions function in individual countries.

Without the benefit of country studies, these scholars were almost forced to assume, and did assume, that the military organizations of Latin America function in largely similar ways when they assume arbitrating or governing roles. The evident fact that military behavior has changed over time as well as the obvious differences among the "typical" Latin American militaries were explained by other means—by referring to such factors as foreign influences, the class origins of the officers, a vaguely defined concept of professionalism, and military strength and firepower—and were accompanied by both friendly and critical interpretations.

Seven of the "old ideas" that have influenced the public image of Latin American military behavior in politics can be summarized as follows:[1]

	FAVORABLE	UNFAVORABLE
(1)	*Historical origins*	
	The armed forces sprang from the wars of national independence and played a unique nationbuilding role.	The armed forces are essentially an alien institution within the nation in quest of a role.
(2)	*Foreign influence*	
	The influence of foreign missions on military behavior in	Foreign missions have strengthened the military and have made it an

[1] Most of the propositions summarized here can be found in one form or another in Lyle N. McAlister, "Recent Research and Writings on the Role of the Military in Latin America," *Latin American Research Review*, II (1966), 5-36; in L. McAlister, "The Military," in John J. Johnson, ed., *Continuity and Change in Latin America* (Stanford, 1964), 158-60; and in Alfred C. Stepan, *The Military in Politics: Changing Patterns in Brazil* (Princeton, 1971), 7-56.

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	FAVORABLE	UNFAVORABLE
	politics has been toward democratic practices.	influence harmful to democratic development.
(3)	*Superpower in politics* The officer corps "intervenes" in political crises as an all-powerful group which provides needed stability to unstable political systems.	The armed forces become politically active for self-interested motives; they repress democratic forces and prevent political development.
(4)	*Civilian attitudes* The military role has wide civilian acceptance as a basic element in the "Iberian" culture patterns of the Latin American countries.	All civilians (except a few vested interests) oppose the use of military force in politics.
(5)	*Equipment and size* Military equipment and firepower promote "professionalism" and keep the soldiers out of politics.	Military size, equipment, and firepower are directly related to military assertiveness and the use of force in politics.
(6)	*Social class* The middle-class social origins of the officers create military support of and zeal for socio-economic reform and even revolutionary sympathies.	The middle-class origins of the officers cause them to defend the status quo and to oppose mass participation.
(7)	*Professionalism* Professional armed forces are politically neutral, and politically neutral forces are by definition professional.	A large and professional military establishment is likely to become involved in politics.

While the debates of the 1960s over the Latin American military were elaborating—and hardening—doctrine on the political roles of the armed forces, a small but growing number of scholars devoted themselves to in-depth studies of individual countries. Recently published information about Latin American military institutions of major countries shows that the country specialists have arrived on the scene in force. The results of their investigations, in-

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cluding books, articles, papers, and dissertations, have by now supplied a large installment of the missing data about the military as "human institutions operating in the context of historical reality"[2] and permit testing old ideas in the light of how the military actually has conducted itself in many different situations.

(1) *The military: a mirror of society*

The reasons why Latin American armed forces play so large and important a role in politics are traced both by critics of the military and their friends to their historical origins—Spanish colonial policy and the wars of independence—and to borrowings from European models by nationbuilding leaders. To the critics, the military establishments of these countries are essentially just another case of the chronic Latin American weakness for institutions not compatible with their own—for French and U.S. constitutions and political forms, Prussian military organization, British naval traditions—and of the unintended results that can follow from foreign borrowings. The adoption of European-style national defense functions and the military apparatus of the nation-state is seen to have introduced an irrelevant and alien "institution in quest of a role," which doomed the democratic, constitutional development of the republics from their early days.[3] Friendly observers, on the other hand, recall the role of the military in founding these countries and view the import of foreign technology as significantly furthering national development.

In fact, present day Latin American armed forces have no more continuity with the colonial or independence eras than do the societies within which they developed. From the end of the wars of independence until the late nineteenth century, the military forces maintained by the national governments of the area were generally small and insignificant organizations except when temporarily expanded to fight international or civil wars. The main function of these poorly organized militia-type elements was border

[2] Robert A. Potash, *The Army and Politics in Argentina, 1928-1945, Yrigoyen to Perón* (Stanford, 1969), vii.

[3] Charles Anderson, *Politics and Economic Change in Latin America: The Governing of Restless Nations* (Princeton, 1967), 26-27, provides a scholarly statement of this view.

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defense and the maintenance of internal security. The instruments of force and violence were held by the innumerable local military chieftains (*caudillos*) and insurrectionists of the postindependence decades as well as by military organizations at the command of the central government; the national military forces were often in competition with irregulars. When "the military" seized control of government, the regime usually represented the union of personalism and violence for the conquest of power. The chief executives of these regimes were essentially national *caudillos*.[4]

Career Latin American military forces of any consequence evolved with their countries from the poverty, caudillism, and anarchy of the early and mid-nineteenth century to the more or less stable condition of effectively unified nation-states, a process that for some countries has extended to fairly recent times. When progress began to accelerate at the end of the last century, the then dominant

[4] How the armed forces were organized in the nineteenth century, the ups and downs of their institutional life, and how they functioned in relation to civilian interests are described, often in considerable detail, in a number of country studies. A wealth of information about the Venezuelan forces and their relation to society during the turbulent and disorderly *caudillo* period that lasted from independence to the early twentieth century is in Robert L. Gilmore, *Caudillism and Militarism in Venezuela, 1810-1910* (Athens, 1964). Abraham F. Lowenthal, *The Dominican Republic: Politics of Chaos*, Brookings Institution Reprint No. 158 (Washington, D.C., 1969), 39-40, 49-53, finds comparable patterns in the Dominican Republic. For the background of the Brazilian national army, its weakness, and inferiority to state militias in the early decades of the republic, and the resulting military-civilian political relations, see June E. Hahner, *Brazilian Civil-Military Relations, 1889-1898* (Ph.D. diss., Cornell University, 1966), particularly chaps. VII and VIII. For Argentine development, see Jose Ferrer, *The Armed Forces in Argentine Politics to 1930* (Ph.D. diss., University of New Mexico, 1966), chaps. I and II. For Colombian development, see Anthony Maingot, "Colombia," in Lyle N. McAlister *et al.*, *The Military in Latin American Sociopolitical Evolution: Four Case Studies* (Washington, D.C., 1970), 129-48. Pertinent background on major South American forces is provided in Liisa North, *Civil-Military Relations in Argentina, Chile, and Peru* (Berkeley, 1966), particularly in chaps. I-III. None of these sources confirm the usual portrayals of an "oligarchic triarchy" (clergy, landowners, army officers) which allegedly employs a well organized, foreign-influenced military arm to impose its authority on national society. The governments in most countries were badly organized, suffered frequent turmoil, and lacked any settled power patterns. The Dominican situation as described by Lowenthal, 53—"an insecure grouping of elite families, a weak and dependent church, and no national military institution . . ."—is perhaps extreme, but it bears at least a family resemblance to many of the Latin American societies during much of the nineteenth century.

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civilian leadership of major South American nations sought to establish professional civilian-controlled forces that would abstain from politics, defend the nation's territory, assert national prestige in the area, and in general affirm their break with a chaotic past.

Although they looked to Europe for models of the "new military," it is clear from studies of particular armed forces that these countries were able to build strong career forces with professional standards only when they had developed comparable civilian institutions. It was not enough for the governments to invite foreign missions to advise their armed forces or to set up advanced schools and establish national conscription. Rather the military institutions developed as part of a sometimes long drawn-out process in which the central government achieved unquestioned authority in national life and the country gained sufficient social and economic resources to afford a military with professional standards. It was also essential for the government to decide that forces of high caliber and high capabilities were indeed a necessity. Hence, the development of military institutions in several countries started very sluggishly, reflecting a lack of will or ability to absorb and to assimilate what foreign technology in this field could supply.

Current studies of particular military institutions, then, show a record of change and evolution mirroring the national societies to which the armed forces belong. They provide no evidence to support a conclusion either that the Latin American military is a "foreign incubus" or that it has played a key nationbuilding role.[5]

(2) *Foreign influence on the military is seldom decisive*

The influence of foreign military aid is generally stressed more by critics than by those friendly to the military. Certainly a large quantity of "foreign aid" has flowed to the Latin American military at one time or another. Foreign military aid clearly has had

[5] How and when the military of some of the larger South American countries made the transition to "developed" status is recounted for Argentina by Potash, 1-28, and by Ferrer, chap. II; for Venezuela by Winfield J. Burggraaff, *The Venezuelan Armed Forces in Politics: 1935-1959* (Columbia, Mo., 1972); for Chile by Frederick N. Nunn, *Civil-Military Relations in Chile, 1891-1938* (Ph.D. diss., University of New Mexico, 1963), chaps. I-II.

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some influence on the military and its political role in the Central American and Caribbean areas. Prior to World War I, the U.S. government was instrumental in organizing security forces in several small countries of the area and these organizations later played a significant part in national politics.[6] In the 1960s, the Soviet government, through massive military assistance to Fidel Castro, helped consolidate extensive social and political changes in Cuba.

Outside the Caribbean, however, the evidence that foreign countries exerted lasting influence through assistance to Latin American military forces is far less convincing. In South America, the armed forces of Chile and Argentina reached professional status more or less simultaneously at the end of the nineteenth century with the same foreign source of advisory missions, Germany. They both established prestige forces, but these two countries subsequently followed very different courses in political and international affairs. The same was true of Brazil, Colombia, Peru, and Venezuela, which received foreign missions, sooner or later built up armed forces substantially beyond the level of internal security needs, and achieved professional levels of institutional development.[7] The countries where foreign aid was limited or lacking and where the

[6] The U.S. role in developing a military institution in the Caribbean area is told in great detail for Nicaragua by R. L. Millett, *The History of the Guardia Nacional de Nicaragua, 1925-1965* (Ph.D. diss., University of New Mexico, 1966), particularly chap. V. Lowenthal, 53, notes the political consequences that followed from U.S. success in organizing the Dominican Guardia Nacional, and parallel cases existed in other Caribbean countries. A comparable review of the functioning and influence of Soviet military missions in Cuba remains to be done.

[7] For a review and evaluation of how German military missions shaped the Argentine military's political attitudes, see Potash, 3-5, 51-52, 117-25, 170-74, and 284. He emphasizes the pervasive extent of German influence in the army, but questions that the "exposure of Argentine Army officers to German military doctrine and training inculcated a disdain for civilian politicians and a hostility to democratic government." As he notes, "In neighboring Chile, German military influence was much stronger than in Argentina prior to 1930, but there the armed forces have accepted civilian rule and have played a much less active role than their Argentine counterparts. Furthermore, the persistence of military activism in the 1960's, long after the waning of German military influence, casts doubt on its significance as a basic cause of earlier military involvement" (p. 284). Similarly the differing political courses of Brazil and Peru, both of which received strong French army missions, can scarcely be explained by French influence on the military establishments of these countries.

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military remained at or below the level of internal security forces, followed similarly divergent paths in politics. Hence foreign military indoctrination can hardly be blamed for the weakness of the constitutional process in much of the area, or conversely, be credited for introducing more democratic attitudes.

In addition to being criticized for distorting natural lines of political development, the military is sometimes seen as overly responsive to foreign manipulation or, conversely, as providing a useful and desirable channel for foreign influence. In general, however, the U.S. government, since World War II the chief source of military aid, has had no more influence in shaping the policies of the host country than in shaping the behavior and capabilities of the Latin American armed forces in politics. The more weakly organized forces of Latin America remain as little able to provide a channel for foreign influence as to form and to advance effectively and consistently their own corporate views (as distinguished from the fortunes of a *caudillo*). The "professional" forces of South America, on the other hand, are by now generally rather resistant to influences from abroad and tend to follow their own well established bent. It is also important that in these countries the air and naval forces have received much of the available foreign aid, while the army, politically the crucial service, is essentially a homegrown force with a character and with policy preferences determined by many, complex interrelations with national life.

Of the major South American armies the Brazilian army has had the heaviest exposure to U.S. influence, beginning with World War II when Brazil was an active ally and sent substantial expeditionary ground forces to take part in the Italian campaign. Yet even in the case of Brazil and even since the army assumed direct rule in 1964, U.S. influence on national policy through the armed forces —either by direct indoctrination or by indirect means—has probably been no more efficient or effective than through other means, such as civilian technicians and businessmen. Moreover, the influence of the Brazilian officers who with U.S. aid carried the Brazilian army from the preprofessional levels of the 1920s to its present status is fast fading as new officer generations rise to the top commands.

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(3) *The military is part of the political system*

Both the military and its critics have spoken as though the armed forces have the political effectiveness of an institutional superpower outside and even above the political system. The officers rationalize their role quite explicitly as protector of the state against the forces of subversion and/or "guardian of the constitution." They regard themselves, on principle, as above "politics," a force external to the political system with a transcendental mission to defend it from subversion. Supporters of the military see it as a "moderating" force, a modern substitute for royal authority or, using another analogy, a kind of "supreme court" in the constitutional system. Critics of the military accept the view that the officers are indeed outside the political system and speak of their "intervening" in politics.

The view that the military intervenes in political life in the true sense of the term—enters the system from outside as a party extraneous to the proceedings and capable of entering and extricating itself from the political process at will—is questioned by scholars who have taken a close look at transactions between civilian political elements and the military and have analyzed the reciprocal pattern of influence over the years. Realistically, the military has to be seen as a force within the Latin American political universe which is highly interrelated with civilian political forces. While the Latin American military is clearly "outside" the political party structures as such, still political parties are not by any means the exclusive centers of activity in Latin American politics, and many disparate nonparty forces belong within the political world. Among a miscellaneous collection of nonparty, "irregular" forces —including unions, student organizations, radical churchmen, economic pressure groups, and foreign governments—which contend in political contests and confrontations, the military is merely one, albeit often the most effective, nonparty force in the political process.[8]

[8] Why, how, and to what effect the military is involved in the political process in particular Latin American countries is fully and effectively treated for the first time in a number of works cited above and in several others cited below. The Potash study of the army in Argentine politics is an exhaustively re-

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The officers are often described as a pressure group within the political system, but although this concept ties them more realistically into the political process, it still fails to do justice to military roles in the political world. In most Latin American countries, the military is politically more strategic and actually or potentially more involved with the centers of power than a pressure group is able to be. Thus the military periodically assumes governing authority, and the center of power upon which it is supposedly exercising pressure becomes occupied by the pressure group itself. When not serving as the key constituency of a military president, it may be a shadow government to a civilian administration, a veto group vis-à-vis an unfriendly chief executive, or the arbiter between the government and opposition forces. Sooner or later, even

searched and clearly presented account of civil-military relations in Argentina during a crucial period in this country's political development. As well as providing the kind of comprehensive and authoritative information never before collected on a Latin American military institution's political roles, it provides a model for future historical work in the field of civil-military relations in the political system. Stepan's review of the military in Brazilian politics treats another major military institution from the point of view of a political scientist, analyzing the military as a subsystem in relation to the overall political system and singling out the political and economic conditions that affect civil-military relations. His book makes use of the unusually extensive and accessible sources of data on the Brazilian military and its relations with the civilian political sector and represents the most comprehensive and far-ranging review in the political science literature of civil-military relations in a Latin American country. Luigi R. Einaudi, the leading authority on the controversial Peruvian military, has drawn upon his forthcoming study of the Peruvian military in politics for two monographs: Einaudi, *The Peruvian Military: A Summary Political Analysis* (Santa Monica, May 1969), and Einaudi and A. C. Stepan, *Latin American Institutional Development: Changing Military Perspectives in Peru and Brazil* (Santa Monica, April 1971), 9-70, 121-32. These two papers place the Peruvian military in terms of its institutional development, political capabilities, and relations to important civilian sectors, and explore officers' attitudes on key national issues, introducing information and theories about their motivations that go far to delimit possible future lines of military political behavior in a particularly interesting but complex and obscure situation.

Other works worth consulting on the place of the military in the political process include: Francois Bourricaud, "Peru hoy-Los militares: por que y para que?" *Aportes*, no. 16 (1970); Burggraaff; Francisco Luis Buitrago, "Politica e intervencion militar en Colombia," *Revista Mexicana de Sociologia*, XXXIII (1970), 491-538; Lowenthal; McAlister, *et al.*; and Philip B. Springer, "Disunity and Disorder: Factional Politics in the Argentine Military," in Henry Bienen, ed., *The Military Intervenes: Case Studies of Political Development* (New York, 1968).

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though "pressure group" may describe part of the range of its activities, the term becomes inadequate. It is also true that conceiving of the military as a pressure group in politics suggests that the Latin American political party and civilian authority systems are much more autonomous and stronger than they are, and that boundaries between the military and civilians within the political system are well defined, which they are not.[9]

(4) *Civilians and military coexist uneasily*

Assertions that the military is welcomed—or is rejected—by civilians when it enters the political arena can easily be proved, given a choice of the right moment in time. In general the critics are right in claiming that civilian politicians seldom welcome military participation and constraints in political affairs. Popular leaders have been ready to establish a one-party system something like the Mexican PRI to curb the role of the military. Other civilian leaders at one time or another have moved to purge the military and subordinate the armies to civilian control by building up police forces, labor militia, or even the air and naval services. In Mexico, Bolivia, and Cuba the existing military establishments were purged or effectively subordinated as the result of revolutions. Yet, despite ample evidence that civilian politicians dislike military activism in politics, nevertheless only in Mexico has the military remained under effective, institutionalized political control for a long period of time, and even in Mexico no one takes any action related to the army without the greatest caution. Elsewhere, except in Costa Rica, competing civilian interests have lacked the unanimity needed to unite and impose a political order excluding the military from politics.

Civilian attitudes toward the military are ambiguous because the majority of participating civilian political forces and interests so far have continued to support the existing order with which the national military establishment is intertwined. Even though the civilians may want to curb military activism, the equally strong wish of many, except in a few countries, to see the military, over

[9] The works on military-civilian relations cited in note 8 illustrate the widely varying style, behavior, and governing coalitions of the military when it becomes active in politics and involved with civilian forces.

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the long run, play some limited political role has left the door open to the officers to resume active participation in politics. This is the case not only in countries where the military has been active in politics, but in countries where its participation has been marginal for many years.[10]

(5) *Equipment and size are no measure of political striking power*

The use of military force in politics is often linked with military firepower to provide some measure of the armed forces' political influence. Actually the relation between the military equipment of the Latin American armies and their political influence is minor and indirect. The same is true of their manpower strength (approximately 700,000, army, navy, air), which represents on an average about one per cent of the Latin American labor force and includes a sizeable number (over half the total) of short-term conscripts.[11] In the nineteen Latin American republics (excluding Cuba), the active-duty officers of the armies, always the political center of these military establishments, number less than 50,000. The core army-officer group of the nineteen republics, if defined to include only the regular army line officers, normally the chief political cadre, is considerably smaller, possibly no more than 30,000 in an area population of about 260 million.

That there is no necessary correlation between equipment and size and military influence and assertiveness in politics is illustrated by the case of Chile, where the size of the armed forces is disproportionately large (half again as much as the area average) and the army well equipped in relation to the country's population, size, and national wealth. Guatemala, where military men are active and influential, has a relatively small armed force, about half the area average in military strength as a percentage of the labor force.[12]

[10] There is abundant evidence of civilian efforts to develop and use military backing in politics, as summarized in John J. Johnson, *The Military and Society in Latin America* (Stanford, 1964), 119-23. For description and analysis of civilian initiatives in particular countries, see Potash, 29-54, 182-85, 283-84; Stepan, chap. III and pp. 187-98.

[11] For estimated strengths of area armed forces, see David Wood, *Armed Forces in Central and South America*, Adelphi Papers, no. 34, Institute for Strategic Studies (London, 1967), 22.

[12] Stepan, chap. I, deals with the question of size as a factor in armed forces' political roles.

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Moreover, prestigious military equipment is generally in the hands of the naval or air forces (chiefly in the larger South American countries), and the naval and air services normally play a very limited role in politics compared with the equipment-poor army. There is indeed a connection between equipment and professional status —a highly professional force will generally have better equipment than a simpler force. Professionalism, however, as noted below, does not necessarily indicate either a low or high level of political activism.

The military has been strong and able to maintain its political role in Latin American national life not because of size or, above a basic minimum, firepower and equipment. Essentially its position has been accepted by the dominant civilian political elements and it has remained—relatively—the best organized and most durable institution in each country over the years, an integrated and somewhat independent sector of the government bureaucracy which is bound together by common professional ties. At the same time, the military has carefully kept the balance of coercive force in its own favor vis-à-vis potential competitors such as the police and party militias.

(6) *Social class does not determine political role*

Explanations of officers' political views more often than not refer to their class origins. When they follow a reformist line, it is explained that they incline toward reformist or radical courses because they are middle class. When they seem to be taking a conservative tack, however, the explanation, particularly from a Marxist view, is that the officers are representing the middle class as a "have" group. Middle-class military activists, in this view, are doing the political dirty work of the middle strata of society, which are too disorganized to represent their own interests and therefore call on the military to protect middle-class ascendancy against the rise of mass political movements. Still other theorists claim that increasing numbers of officers are coming from the working classes; hence the military soon will be ready to support truly revolutionary programs.

The officer corps in fact has middle-class social status in most countries and appears to be largely middle or lower middle in class

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origins.[13] However, the argument that the officers' social level has changed and that the middle-class status and/or origins of the officers have caused the military to adopt middle-class programs and abandon the old ways they followed when officer candidates were aristocratic or upper class by birth and so were allies of reactionary oligarchs and other vested interests such as the church, is not supported by the evidence. The bulk of the officer corps has long been drawn from the middle or lower middle layers of society. In contrast, in the civilian sector in recent decades, the dominant civilian political leadership has passed from the hands of largely upper-class groups, where it rested prior to World War I, to middle-class politicians.

The military's changed relation to civilians doubtless reflects altered social patterns—the changing social origins of the dominant politicians as well as the broadened social and political base of civilian politics. It is far from true, however, that the military has made itself a praetorian guard for the middle-class political leadership or, on the other hand, that their middle-class origins infuse military officers with zeal for reformist or even radical measures. There is no middle-class political program which can be defined and related to tendencies of the politically activist officers. Broadly comparable though attitudes of the military and civilian middle-class majority may be on the larger issues of economic development, education, or agrarian reform, such attitudes have little value in explaining the officers' political course.[14] Numerous intervening

[13] Despite frequent assertions about the determining effect of class origins of the Latin American officers, specific data about the class makeup of the officer corps remains somewhat limited. Information on this subject suggesting the middle-class origins of the officer corps is provided in McAlister *et al.*: for Peru, 33-34; for Argentina, 97; and for Colombia, 158. See also Stepan, chap. III; and Roy Allen Hansen, *Military Culture and Organizational Decline: A Study of the Chilean Army* (Ph.D. diss., University of California, Los Angeles, 1967), 206. "Military families" appear to be an important source of recruitment in Argentina, Brazil, Chile, and Colombia. Of students matriculating at the military academies in the mid-1960s, 32.5 per cent were of military families in Argentina, 34.9 per cent in Brazil, and 23.1 per cent in Colombia. The Hansen study, based on a sampling of retired generals, shows that fathers' occupations were "military" in 27 per cent of the sample.

[14] The lack of relevance of "middle class" orientation of the officer corps to its political roles is assessed by Edwin Lieuwen, *Generals vs. Presidents, Neo-Militarism in Latin America* (New York, 1964), 104; that "institutional identification is so strong that it obliterates any meaningful identification with

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factors of equal or greater importance invariably influence military forces when they mix in politics, most notably the officers' feeling, of varying degrees of strength, of being an institutional elite. Hence, the middle-class officers have chosen their allies and policies from a wide spectrum, ranging from organized labor to upper-class interests, at one time or another in recent years, much as middle-class political leaders have done. The officers have been less revolutionary than some middle-class leaders and more so than others.

In certain countries and under special conditions, class attitudes, along with other factors, may contribute to the political viewpoints of the military. In Peru, for example, where the social and financial elites have remained aloof from the rest of society and have even on occasion flaunted their presumed racial superiority, the humbler social origins and middle-class status of the officer corps have contributed to the military's suspicion of the upper classes.[15] Even in Peru, however, this class factor alone is insufficient to explain military political behavior. It is one ingredient among the many that are screened through the military institution and together account for the character and performance of the officers' political role.

(7) *Professionalism does not correlate with military activism*

The concept of professionalism has been used as frequently as social origins to explain the military's relation to the political system. Professional military men who abstain from political activism and tend to defend the constitution are often defined as nonmilitarists. Likewise officers whose behavior is apolitical are said to be this way because they are professional and politics is repugnant to them. Other definitions of professionalism stress the officers' attachment to military missions and call attention to the role of equipment in building professional morale. More recently another school of thought has emerged which favors the theory that professionalism leads to higher levels of military activism.

In order to have meaning in the Latin American context, military professionalism needs to be understood as a level of institu-

civilian social groups," is confirmed by Stepan, chap. III, with extensive data and analysis for Brazil.

[15] Einaudi, 5-6.

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tional development meeting certain standards of career development, specialization, independence, and organizational stability. Ascent of the armed forces to this level is only possible in national systems of some scale and a fairly advanced stage of social and economic progress.

The closest approach to professionalism among the Latin American armed forces is found in the larger South American countries—Argentina, Brazil, Chile, Colombia, Peru, and Venezuela. These countries have forces with, by Latin American standards, high professional qualifications. The graduates of their military academies enter a career which normally occupies them in professional pursuits on a full-time basis. There is reliable budgetary provision for pay, equipment, and operations. Advanced training for officer promotions is the rule. Norms established by law, and usually observed in practice, rule promotion, assignment, and retirement in their officer corps.[16] Reflecting these procedures, the rank pyramid of the officer corps in the larger South American countries tapers at the top in contrast to a bulge at the top of the rank structures typical of simpler, nonprofessional forces.

In this sense, professionalism has been very important to the political style and capabilities of the military, but has no direct correlation with levels of military activism in politics. It was this lack of direct correlation that disappointed and frustrated the governments of the leading South American countries that invited foreign missions to train their armies and "civilize" them so that they would abstain from political action. How little direct relationship there is between professionalism and levels of activism is revealed by the mixed record of the institutionally well developed South American forces—on the one hand the high activism of the Argentine, Brazilian, and Peruvian military and on the other the low

[16] The Argentine sociologist, Jose Luis de Imaz, uses these tests of military transition to professionalism in Imaz, *Los Que Mandan* (Buenos Aires, 1964), 52. For descriptions that point up contrasts between the major South American forces and simpler organizations of other Latin American countries, see the Argentine study by Potash, 86-103; Stepan, chap. III, commenting on "bureaucratic norms in the military and political activism"; Lowenthal, 39-42, on the Dominican Republic; Richard N. Adams, "The Development of the Guatemalan Military," *Studies in Comparative International Development*, Washington University, Social Science Institute, Sage Publications, Vol. IV, no. 5 (St. Louis, 1968-1969); McAlister, *et al.*, 232-34, on the Mexican army.

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activism of the Chileans and Colombians. Among the "less developed" forces there is a similar lack of correlation as shown by the low activism of the forces of Costa Rica, Mexico, and Uruguay and the high activism of the other ten forces in the subprofessional category (the militaries of Bolivia, Ecuador, Paraguay, the Dominican Republic, El Salvador, Guatemala, Haiti, Honduras, Nicaragua, and Panama).

Conclusions

The debates of the 1960s regarding the political roles of the military were carried out on the wrong terrain with the wrong weapons. Recent country studies of Latin American armed forces indicate that the ideas about the military popularized then, both pro and con, misread history, overrated external influences, misinterpreted the character of the military institution in Latin America, and failed to identify characteristics of the armed forces that affect their political action. Specifically, the recent country studies find that military men mirror their societies. Over the long run they resist foreign influences. They exhibit a high degree of adaptation to domestic political life; they are not self-determining political entities or superpowers, but interact closely with the civilian sector. Their political behavior is unrelated to the absolute or relative size and firepower of the armed forces, nor does it seem to be related to their class origins or to degrees of so called professionalism.

More positively, the new literature on national military institutions has begun to provide an understanding of what the Latin American military is like and how its members behave. In the no-man's-land between the study of political and military institutions the historians have occupied ground in key sectors previously covered only by distant observation and general studies, and the political scientists are entering with works of increasingly high quality. While these new studies have not yet established a comprehensive and coherent theory of military behavior in Latin American political life, they do place it more firmly in the domestic social and political context. They tell the reader (1) to scrutinize the institutional character of the military as it has developed over time; (2) to place the military institutions within their total national framework; and (3) to observe the interplay of the military

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with society in the present day. As noted in the preface of one of these studies, the approach adopted is "one that focuses on the real-life figures who have led the armed forces, on the details of institutional development, and on a broader spectrum of political behavior than the occasional coup."[17]

[17] Potash, vii.

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The Automobile Industry Since 1945 by Lawrence J. White: A Discussion

The editors of the *Political Science Quarterly* invited representatives of business, labor, and consumer organizations to submit reviews of *The Automobile Industry Since 1945* by Lawrence J. White (Cambridge, Mass., Harvard University Press, 1971; xii, 345 pp. $12.00). The following reviews were prepared by Lowell Dodge, director of the Center for Auto Safety, a private, nonprofit group based in Washington, D.C., that works with Ralph Nader; H. L. Duncombe Jr., director of economic studies for General Motors Corporation; and George Schwartz, assistant director, research department, United Automobile, Aerospace and Agricultural Implement Workers of America—UAW.

I

As a cause of violent death, automobiles have no challenger in this country. The toll is amply forecast before the fact and broadcast afterward. It is tallied (55,000 in 1970, more than the total of Americans killed in all the years of war in Vietnam) and the tally is given considerable publicity. For every death on the highway, another 80 motorists and pedestrians are injured there, almost half sustaining injuries which are disabling beyond the day of the accident. Highway deaths and injuries are within the experience directly or indirectly of millions of citizens.

The tragedy is known; how it is interpreted is another matter and one which has remained the principal obstacle to the rational selection of countermeasures and their implementation. Conventional

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wisdom on safety strategies comes from the auto industry and from the traffic safety establishment it supports. It directs the focus to the cause of the accident, usually identifying an errant driver, who is then regarded as the cause of consequent injuries or deaths. To cut the carnage, according to the conventional view, improve driver performance.

Attention is thus diverted from the immediate causes of most deaths and injuries inside and outside the car: hostile dashboard projections, penetrable windshield glass, harpooning steering columns, mirror and visor fixtures, pedestrian-gouger snouts, and other design horrors. The outrage is that the severity of injuries can be greatly reduced, and many deaths prevented, simply by the application of available safety technology to vehicle design. Also underutilized are crash avoidance technologies as well as designs to eliminate the potentially lethal aspects of the construction of roadside objects. Countermeasures focusing on needlessly dangerous vehicle designs have a relatively high benefit-cost ratio for several reasons. First, it is an infinitely more manageable task to influence the behavior of the twenty or so manufacturers who design cars for sale in the U.S. than it is to influence the behavior of 111 million licensed drivers. Driver performance is affected by complex characteristics of mind-set, experience, and habit patterns not easily influenced by public programs. Second, vehicle safety design is a highly appropriate object for a federal regulatory process. The fifty states can be spared the effort. The problem is national in scope; neither its definition nor its solutions are subject to differentiation on a geographic basis. A federal rulemaking process which sets safety standards for new cars, begun in 1966, has already shown signs of effectiveness (the death toll dipped in 1970), even though federally required safety features are now incorporated in only about half of the vehicle population. Third, many safety designs involve merely substituting a safe design for an unsafe one (e.g., dash panels, hostile projections on the front end), imposing no major production costs on the manufacturers. Finally, fifty years of spending on the conventional approach has had no noticeable impact on the number or rate of fatalities.

This interpretation of the tragedy makes it difficult to justify a study of the auto industry which fails to arrive at conclusions highly critical of the actions of the industry.

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Lawrence White's attempt to describe and explain the actions of the automobile industry falls short of providing a sense of basic understanding of the industry's conduct. By expressed intent, and in execution, it is primarily an economic study, using the industrial organization tools of structure, behavior, and performance as the basis for analysis. While the book's application of economic tools of analysis is flexible and at points even creative, some of the explanations offered for marketing and pricing practices seem incomplete, and his prescriptions timid. The most serious shortcoming is the tendency to treat industry behavior on matters of public concern, such as auto safety, as afterthoughts. This is perhaps dictated by the selection of an economic perspective. But the apparent result in the discussion on safety is that much is omitted and what is said receives inadequate treatment. According a higher priority to issues such as safety and carrying the analysis of the more purely economic aspects of the study closer to completion might well have led the author to a bolder and more critical formulation of his conclusions and recommendations.

The core of the book examines certain aspects of the industry in an economic framework: scalar economies in auto production, conditions of entry, the structure of demand, pricing, and distribution. It produces several intriguing observations on industry behavior, but at some points, the author does not appear to draw as much significance from his observations as he might.

Take for example a most interesting discussion of planned obsolescence. Have the auto companies purposely designed cars for a shorter and shorter useful lifespan to speed up replacement sales? Possible evidence of the decreasing durability of cars is graphically presented in a table showing for each year over a fifteen-year period the numbers of seven-, eight-, and nine-year-old cars still on the road, as a per cent of their original production. The table indicates that 80.70 per cent of cars made by the big three (GM, Chrysler, and Ford) which were nine years old on July 1, 1955 were still on the road on that day. On July 1, 1967 only 55.23 per cent of the nine-year-old cars made by the same companies were still on the road. The decrease occurs with a regularity that makes it look very much as if it were planned that way, or resulted cumulatively from cost-cutting programs. Analysis of further data confirms the trend toward decreased durability. But because other factors might also ex-

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plain the trend (e.g., reduced consumer interest in stretching out the life of cars as a result of rising income over time) and because efforts to sort out the effects of these additional factors were unsuccessful, no answer is reached. One is left with the sense that the author reached the edge of a significant conclusion, but dropped it because of the difficulties encountered in pushing the analysis to completion.

In the development of the analysis, it matters little, however, that not much is made of planned obsolescence, for what immediately follows is a description of an even better strategy to maximize the acceleration of replacement sales. (It does not strike the author that the industry might be pushing both strategies simultaneously.) The second strategy is to convince owners to trade for a new car by leading them to believe that the "economic usefulness of their cars has diminished before the actual physical usefulness ends. . . ." The convincing could be based on new technological advances being introduced into new car models every few years or by frequent styling changes. The author explains that high risks attach to the use of technological advances in auto design for this purpose; it follows that auto design has been slow to adopt new technology. But there is ample evidence, well presented in the discussion, to support the existence of the style-change strategy. Moreover, the strategy is supplemented by the purposeful proliferation of model types, which totaled 370 in 1967, to induce owners to trade up to something a little more glamorous.

But again the analysis seems to fall short. The author estimates the cost of frequent model changes at $130 per car. (Note that this is the cost to the manufacturer, which the author does not translate into consumer cost, presumably a much higher figure. Yet in assessing the cost burden of safety devices in a later chapter, he uses the higher cost-to-the-consumer figures.) He asks whether excessive resources are devoted to styling, model changes, and model proliferation. Unwilling to interject any policy judgments, he concludes that this question should be left to the market and adds that he believes the allocation accurately reflects consumer preferences. Manufacturer costs of $130 per car will buy almost all of the safety and pollution improvements currently proposed for cars. He does not ask whether consumers might be willing to trade frivolous styling changes for improved safety and pollution performance, if nothing

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were added to the retail price of the car. An earlier chapter on the structure of demand indicates a lack of consumer interest in "basic transportation," but how much of that preference is just that—"structured," by extensive advertising? Perhaps demand might be restructured if auto advertising encouraged potential buyers to look for functional qualities as vigorously as they are now cajoled into buying for style, status, and image.

More fundamentally, the author fails to carry his perceptions on style-change practices into his discussion of barriers to entry into the industry. He might have posed the question: do frequent style changes constitute an unfair trade practice by raising barriers to entry to the point where they are insurmountable? He estimates the total capital required for an entrant at about $1 billion at a scale of 800,000 units per year. This estimate appears to incorporate the cost of capability for annual styling changes. But he does not consider to what extent this total estimate could be reduced if style change were not the prevalent industry practice. A recent *Yale Law Journal* study [1] put the cost of entry into the industry in 1970 at $779 million (at a scale of 200,000 units per year) and estimated that the costs of annual style-change capability account for fully $724 million, or more than 90 per cent of the total, assuming the existence of independent parts manufacturers and independent auto distributors. (At 800,000 units the percentage would be cut to about 75 per cent, still very high.) The study argues that frequent-style-change capability requires efficient assembly, integrated components-production facilities, integration forward into a nationwide distribution network, and extensive retooling and readvertising costs, otherwise not absolutely necessary. White might not agree with the conclusions of the article, but it raises important questions he fails to discuss adequately.

A further practice is neatly explained in a section of the chapter

[1] "Annual Style Change in the Automobile Industry as an Unfair Method of Competition," *Yale Law Journal*, LXXX (1971), 567. The article details market power accruing to a company which can afford frequent model changes. GM's market share in 1922, the year before it adopted a style-change policy, was 13 per cent, and Ford's 51 per cent. By 1927, GM's share was up to 43 per cent, and Ford, which did not adopt the style-change policy, dropped to 9 per cent by the same year. Ford finally went along and within two years its market share bounced back to 31 per cent. While aggressively pursuing annual style change, GM, Chrysler, and Ford increased their collective market share from less than 65 per cent in 1923 to more than 90 per cent in 1935.

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on prices and pricing policy—the institution of dealer sales contests prescribed by the manufacturer, usually after the midpoint of the model year, on selected models. They are contests in name only, for rewards are granted in most instances merely for meeting sales quotas previously imposed by the manufacturers on the dealers. Rewards take the form of a rebate to the dealer of a portion of the price paid by the dealer for the cars sold during the contest period. The rebates enable the dealer to be more flexible in cutting the retail prices on affected cars, hopefully leading to increased sales. White points out: "In effect, the manufacturers are adjusting their prices to the changing conditions of the market over the model year. To a large extent, they are acting as discriminating monopolists, segmenting their own market over time. . . ." Quite appropriately, he then asks why this price cutting is accomplished by wholesale price rebates rather than by straight changes either in the wholesale or retail list prices. The answer is instructive. It is done to effect price changes without attracting the attention of the press and Washington, thus avoiding explanations to reporters about poorly selling models and inquiries from public officials on larger price increases at the transition between model years.

One aspect of industry behavior, corporate secrecy, is encountered frequently in the analysis, but identified euphemistically as a lack of information. The author chides the industry for hiding behind consolidated balance sheets, but is content to suggest only that the Securities and Exchange Commission improve its financial reporting requirements. Lack of information impedes the analysis at several points and forces the author to bridge gaps with phrases such as, "In this author's opinion . . ." and "This author believes that. . . ." One might have hoped for at least a low-keyed expression of outrage at an industry practice directly affecting the author's efforts.

On the subject of vehicle safety, the book omits mention of industry performance, which is highly outrageous. The instances cited here are for the most part current; equally cogent instances were available during the period covered by the book.

Well over one-third of all domestically produced motor vehicles have been recalled for possible safety defects since the government began keeping track in 1966. The total now exceeds 24 million. Some have been recalled several times. Many of those recalled are

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found not to contain the defect, but many with extremely hazardous safety defects are never recalled because of inadequate public machinery for investigating reported defects. General Motors deserves credit for deciding to announce recalls when it learns of its errors (GM recalls account for almost three-fourths of the total), but one wonders why the errors are so numerous in the first place.

General Motors comes in for high praise in the book. GM possesses "superior management" and "confidence in its own management and engineering," and at one point it is let off the hook from a possible miscalculation, deemed "not likely from a corporation with the reputation of efficiency that General Motors has." What kind of engine of efficiency is it which in 1971 alone announces a defect in 600,000 Corvairs (poisonous fumes entering the passenger compartment through the heating system), but leaves owners to pick up the tab ($170) for making the correction where needed; agrees to two major recalls in advance of a formal government finding of defect to avoid such a finding and its attendant liability implications, crucial to private litigation by injured owners; underplays the seriousness of a defect in letters notifying owners, who then wonder if it is worth the trouble to visit the dealer for an inspection; and subjects its attorneys to disbarment proceedings in the state of Michigan by permitting, or ordering, them to suppress GM's own adverse test reports on the handling problems of the Corvair to prevent the reports from surfacing in court.

Ford, Chrysler, American Motors, and GM all market cars delivered to retail buyers with an average number of 24 defects each, some safety related. The costs of completing the assembly of new cars are neatly shifted to owners by means of poorly performed warranty service, refusals of warranty coverage, and ineffectual dealing, with the consequent frustrations and complaints. These and other stories are not told in the book.

In its brief excursion into safety, the book makes serious omissions. In describing the scope of the problem, deaths are listed, but not the millions who are injured, including approximately two million disabled beyond the day of the accident.

The author concludes that the industry has indeed moved slowly in its response to public concern over its performance on vehicle safety. It thus seems strange that this conclusion is followed by a recommendation which places the burden not on the poor perform-

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ers, but on consumers, by proposing that the problem be approached by arming prospective purchasers with comparative safety information on cars to permit informed decisions in the marketplace, ultimately affecting design decisions. Such a program has to date a two-year history of dismal failure. The agency administering the program has decided to concentrate on standards which seek to ensure that all buyers can assume a minimum level of safety in any new car they decide to purchase.

The author does reach some strong conclusions. The industry has earned excessive profits; it has been slow in developing vehicle technology; and it has hesitated in responding positively to the questions of safety and pollution. His prescription: more vigorous antitrust action, including a wave of horizontal dissolutions to create a ten-firm industry. But historically, antitrust has not proved effective; holding to its value is to perpetuate an illusion. What is called for in the case of huge, nationwide monopolies and oligopolies is, first, a redefinition and, second, a new approach. Call them what they are, public entities, and then make explicit their public responsibilities. A possible means of bringing effectiveness to such an approach, which deserves added attention in the academic community, is federal incorporation. Socialism is not necessarily intended: competition and corporate democracy would best stand as the guiding principles for the issuance and renewal of federal corporate charters. Detailed regulation, such as rate making, would not be a part of the effort, so that industry lobbying and dependence would not be invited as they are in most regulatory agencies. Teeth would be provided by scaled penalties for violations of charter provisions, citizen access to information and to decision makers, and provisions for shareholder and citizen suits.

However, to support the case for effective reform, we need more vigorous and critical analysis of industry practices than we find in White's study. He has bent too far backward in his effort to achieve scholarly detachment and dispassionate analysis. The appearance of a balanced presentation was perhaps more important to him than telling it like it is.

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II

In the preface to his study, Lawrence J. White writes: "This book was not conceived of as a vendetta against the industry or as a whitewash but, rather, as a scholarly attempt to describe and explain the actions of this very important American industry" (p. vii).

I believe that for the most part he succeeds in his descriptive summary of the industry. Where he fails—particularly in his final two chapters, "Performance: An Evaluation" and "Recommendations for Public Policy"—it is because the author goes well beyond his description of the automobile business, makes unsupportable recommendations, and does not spell out the ramifications of his proposals.

More specifically, Dr. White concludes with the opinion that the performance of the industry would be improved if there were a larger number of domestic competitors. To achieve this, he would have the existing three largest manufacturers divided into, possibly, seven companies. There is nothing in the history of the automobile industry in the United States or in Dr. White's book which supports this recommendation as desirable public policy. In addition, there are critically important questions concerning the implementation of such a policy which the author dismisses with virtually no comment. In fact, he expressly states that such problems of implementation are not his concern: "This author prefers to adopt the attitude that where the major costs, benefits, and economic externalities of a particular policy can be identified, policy recommendations should be made and, hopefully, implemented; *and the burden of proof should rest on those who would argue that the unnoticed ramifications cast doubt on the wisdom of the policy*" (p. 280, emphasis added).

This approach to a major segment of the American economy, involving the welfare of millions of people, is surprising, particularly in a book that purports to be a scholarly work—it hardly inspires confidence in the author's recommendations.

The closest Dr. White comes to an identification of "major costs, benefits, and economic externalities" of divestiture are found in his chapter on performance. In this chapter, the author alleges that ex-

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cess profits characterize the industry.[1] Any difference between the long-term rate of profit in an industry and long-term rate of interest is defined as "excess profit." No one, of course, knows what the long-term rate of interest is. But assuming that, as a substitute, some reasonable proxy were agreed on, the probability is that large sectors of industry in the United States—distribution and service as well as manufacturing—would be condemned.

Furthermore, the model is static, charged with unrealistic assumptions which do not describe the highly dynamic real world. Why is it that the average rate of profit was much higher in 1947-49 when the automobile industry had nine firms than it was in 1965-67 when domestic passenger car producers numbered only four? Why is it that during almost any time period selected, some producers in the industry had virtually no earnings while others were significantly above the average? Dr. White's analysis begs most of the critical questions concerning the profit performance of the industry.

Beyond this, there are the well recognized difficulties in linking available accounting data as to profits with the economist's concept of profit.[2] Dr. White refers to none of these problems. He does, however, make one bizarre adjustment to the data. He argues that some excess profits have gone into wages as the result of the UAW's bargaining power. Industry profits, he alleges, are thus understated. Not only is this approach very crude, but there is no reference to differences in productivity between different industries which, in both theory and practice, account for much, if not all, of observed wage differentials.

Dr. White also discusses the automobile industry's performance in pollution control and auto safety and concludes that the industry's activity in these areas of public concern is "not very impressive." His factual support for such a conclusion is meager, at

[1] Using a study by Gordon R. Conrad and Irving H. Plotkin, "Risk/Return: U.S. Industry Pattern," *Harvard Business Review*, XLVI (1968), 90-99, White comes to this conclusion even after adjusting for the high risks in the industry. A later, and much more substantial study by I. N. Fisher and G. R. Hall, "Risk and Corporate Rates of Return," *Quarterly Journal of Economics*, LXXXIII (1969), 79-92, puts risk-adjusted auto industry profits in eighth place—fourth from the bottom—in the eleven industry groups studied.

[2] See Yale Brozen, "Significance of Profit Data for Antitrust Policy," *Antitrust Bulletin*, XIV (1969), 119-39.

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best, selective, and often inaccurate. Worse, his study provides no understanding of the way a free market system works in these areas.

A competitive free market system provides the goods and services that consumers, in seeking to satisfy their individual priorities, demand. Beyond this, of course, society, through political processes, superimposes its priorities on those of the individual. These social demands to eliminate so called externalities—such as air pollution—are by definition not market motivated. Moreover, clean air or clean water or noise reduction have the aspects of public goods. If they are provided for one, they are provided for all—the marginal cost for an additional user is zero. Consequently, no individual purchaser feels he has to, or is willing to, pay for such items.

When the social demand to eliminate such externalities by nonmarket-motivated methods developed sufficiently, the industry responded. These efforts are continuing. They have been well documented and no reiteration is required here. Against this background, the performance of the industry in the area of automobile emissions control has been particularly impressive. With regard to these control devices, Dr. A. J. Haagen-Smit, whose research at the California Institute of Technology first identified vehicle emissions as a contribution to the formation of smog, reported to the Society of Automotive Engineers early this year that "we are so far over the hump, I am beginning to lose interest."

Efforts in the area of auto safety have a far longer history than emission control programs. The notion that they began with recent federal legislation is incorrect. From the earliest days of the industry, the safety of the vehicle concerned most drivers and has been a primary objective of industry engineering and design efforts. Historically, most auto manufacturers have concentrated on the fundamental safety attributes of the vehicle—attributes which, for the most part, we now take for granted. I have in mind such qualities as improved vehicle weight balance, reduced center of gravity, structural strength of the body and chassis, brake and steering response, and lighting. The energy-absorbing steering column and side impact bars, which in General Motors' case were adopted voluntarily, are further recent evidence of this concern for safety.

In cooperation with dealers, manufacturers also have actively

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supported driver education by supplying vehicles for school use. Moreover, three decades ago at the 1939-40 World's Fair in New York City, General Motors' pioneering exhibit pointed the way toward modern high-speed freeway construction. While it is difficult to assign degrees of importance to the individual factors contributing to highway safety, there is little question that modern freeway design has made a large contribution.

As a result of these efforts over the years, progress has been made in reducing the automobile fatality rate. The average highway death rate for the period 1923-27 was 18.2 per 100 million vehicle miles; in 1971, it was down to 4.7—the lowest rate on record. Highway travel in the United States also is demonstrably safer than in any other major industrialized country. For example, the latest fatality rates for West Germany, France, and Japan are more than twice those of the United States. The number of lives lost in this country is still much too high, but it is not correct to assert that the industry has been laggard in its response to the issue.

If it is Dr. White's contention that a more effective contribution to improved safety or to reduced emissions would have been made by a larger number of automotive firms, he surely must at least supply a rationale for such a conclusion. I find no evidence even hinting in this direction anywhere in his book.

Dr. White ends his analysis of industry performance with a discussion of company size. He believes that the largest volume economies are found in metal fabrication and that the best sheet metal stamping dies have an effective life for the production of up to 400,000 units. Dr. White states that since it would be risky for a single company to produce only one kind of automobile, two types would be needed. Therefore, in his view, an efficient firm size would be some 800,000 units of yearly production. With an 8 million unit domestic passenger car market, he thus concludes that up to ten independent producers could be justified.

This is a curious analysis. Why should anyone assume that each firm could be expected to share the market equally. In the past, as well as currently, there are manufacturers selling both more and less than 800,000 units. One wonders how Dr. White would provide the additional unit demand required by his formula for those selling less. Or having done so initially, does he have any illusions that over time the more successful firms would not outstrip

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the others. With respect to these marketing realities, both the analysis and the proposals are simply naive. As for the 800,000-unit figure itself, Dr. White fails to note that while survival is a necessary condition for investing in a business, it is hardly a sufficient one.

Finally, the size and numbers exercise appears to be based on the assertion that the effective life of a stamping die is limited to 400,000 units. Even if such oversimplified quantification in a highly technical area were acceptable in principle, Dr. White is misinformed. Even for some of the less durable die steels, let alone for exotic materials or even familiar air-hardened steels, millions of units, not 400,000 is the volume limit. On some dies the limit has not yet been reached—if indeed any exists.

Dr. White neither identifies "major cost and benefits" in relation to firm size, nor does he demonstrate that cost would be less and benefits greater if firm size were smaller. There are substantial reasons for the view that the opposite is closer to the truth. His policy recommendation for an artificial division of the industry is unsupported and, in my judgment, unsupportable.

His uncritical implicit reliance on the threadbare argument that the effectiveness of competition is directly related to the number of competitors, also is very surprising in light of the growing body of literature in contemporary economics which questions or defies this assumption. As far as one might know from a reading of Dr. White's book, this solid body of economic analysis which runs contrary to his assumptions about concentration doesn't exist. Could we not expect that for a text which seriously proposes such drastic and untried remedies, we would have had a thorough analysis of the pros and cons of the economic theories which purport to support such remedies. Would Dr. White choose to simply ignore arguments such as the analysis of Dr. Harold Demsetz, which concludes: "The important point that needs stressing is that *we have no theory that allows us to deduce from the observable degree of concentration in a particular market whether or not price and output are competitive.*"[3]

[3] Harold Demsetz, "Why Regulate Utilities?" *The Journal of Law and Economics*, XI (1968), 59-60 (emphasis in the original). See also, E. H. Chamberlin, "Product Heterogeneity and Public Policy," *American Economic Review*, XL (1950), 86-87; E. H. Chamberlin, "Measuring the Degree of Monopoly and Competition," in Chamberlin, ed., *Monopoly and Competition and Their Regulation* (New York, 1954), 262; M. A. Adelman, "The Antimerger Act, 1950-

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The automobile industry is a good example of Demsetz's point concerning the irrelevance of numbers to the degree of competition. But beyond this question of irrelevance, Dr. White among others, fails to give adequate attention to at least two competitive forces that have importantly shaped the industry—the competition of used cars and foreign imports.

The interaction in the marketplace between new and used cars is a continuing and pervasive fact of life in the auto business. In satisfying the demand for private transportation, a large stock of used cars offers the potential new car buyer a wide choice of alternatives. Direct price competition between new and used cars extends over a broad range of prices, from $1,500 to over $3,500. The competitive pressures from used cars elicit continuing product improvement by new car manufacturers and are another dimension of competitive restraint on new car prices. The fact that in any given year new car sales account for only about one-third of total new and used car sales is one indicator of the strong position of used cars as a highly competitive supplier of automobile transportation service.

Nor does merely counting the number of producers recognize the full significance of the competition of foreign cars in the United States. New foreign car registrations in the United States last year accounted for about 16 per cent of total registrations compared to

60," *American Economic Review*, LI (1961), 238-41; Yale Brozen, "The Antitrust Task Force Deconcentration Recommendation," *The Journal of Law and Economics*, XIII (1970), 292; John S. McGee, *In Defense of Industrial Concentration* (New York, 1971), 135; Jules Backman, testimony before the U.S. Senate Judiciary Subcommittee on Antitrust and Monopoly, *Hearings, Economic Concentration*, 89th Cong., 1st Sess., part 2, 1965, p. 561; Corwin D. Edwards, "Control of the Single Firm: Its Place in Antitrust Policy," *Law and Contemporary Problems*, XXX (1965), 465-66; Carl Kaysen, testimony before the U.S. Senate Judiciary Subcommittee on Antitrust and Monopoly, *Hearings, Economic Concentration*, 89th Cong., 1st Sess., part 2, 1965, pp. 552, 556; Almarin Phillips, "Corporate Mergers, Industrial Concentration and Public Policy," *Wharton Quarterly*, III (1968), 23; Eugene M. Singer, "Industrial Organization: Price Models and Public Policy," *American Economic Review*, LX (1970), 98; J. Fred Weston, "Changing Environments and New Concepts of Firms and Markets," in Conference on Antitrust Issues in Today's Economy, *New Technologies, Competition, and Antitrust* (New York, National Industrial Conference Board, 1970), 14. A recent nontechnical account is found in Gilbert Burck, "The Myths and Realities of Corporate Pricing," *Fortune*, LXXXV (1972).

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4.9 per cent in 1962. The share of total sales now being obtained by some imports exceeds that of some domestic cars. There is no doubt that the marketing efforts of foreign producers have been successful. They have used advertising effectively to strengthen their competitive position. In addition, it might be added that contrary to much theoretical speculation, they have found that the existing system of dealer franchises raises no barrier to entry. They have significantly increased the number of their dealerships in the United States by selling through existing dealers on a dual basis and by establishing new franchises. The record is clear that the dealer franchise system has been an aid to competition—not a barrier.

In summary, Dr. White does present a descriptive study of an important segment of American industry and in so doing, he has made a contribution. However, it should be emphasized that no cohesive analysis ties his data together and as a result, his evaluation of industry performance is incorrect. His policy recommendations are unsupported. In particular, the recommendation that the industry be segmented is based on an artificial and simplistic structural model of market behavior—a model which, even if believed, in no way demonstrates that seven or eight automobile producers are better than four—or, if foreign producers are included, that seventeen or eighteen are better than fourteen. The static model, based on industry structure that Dr. White unquestionably accepts, is an inappropriate tool for the assessment of competition and performance in the automobile and probably most other industries.

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III

Given the urgent economic and social issues which automobile industry practices have created, Lawrence J. White's book, the first lengthy treatment of the economics of the industry in many years, is certainly timely. No other industry is so large and powerful or has as much potential for economic and environmental damage. Other products pollute, but only the motor vehicle could be the ma-

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jor nationwide source of air pollution. A faulty widget may annoy but a faulty car can kill. Other businesses also neglect low-profit in favor of high-profit products, but only the auto industry could thereby cause such a devastating adverse shift in the foreign trade balance. In short, business practices which, though questionable, are at least tolerable in other industries can have the most dire consequences when permitted in so important an industry as the auto industry.

Much of White's book is devoted to an "industrial organization" study of its subject. It describes the technology of auto manufacturing, the process of design and tooling, the economies of scale, and the economics of distribution. The basic theme which unfolds is the gigantic amount of capital required for success in the industry. To have more than an outside chance of successful entry, a new firm would have to develop two distinct lines of cars, each with its own dealer organization and each with annual sales of 400,000 units. The cost of the initial investment would approach one billion dollars, not including the separate investments of thousands of franchised dealers. Add to this the need to capture as much as 8 per cent of the market and the track record of previous attempts at entry, and the four present members of the industry are secure in the knowledge that there will probably never be a fifth.

The foregoing is necessary background to the balance of the book. Having established that the industry is indeed a "classic case of a tight oligopoly," a private club which in the absence of price competition can pretty much make its own rules, White goes on to describe how the structure of the industry has influenced its behavior toward consumers, the public, and government. He finds the industry's behavior wanting in nearly every respect: "Strict price leadership is followed. . . . Model changes are stressed as a way of inducing rapid turnover of cars in a replacement market. The industry has earned excess profits . . . has been slow in developing automotive technology . . . has hesitated in responding positively to the questions of safety and pollution . . . has been slow to respond to some of the changes in consumer tastes."

Each of these findings is impressively supported with fact and argument. We learn that GM sets prices so high as to yield a 20 per cent return on investment. The other major producers, though

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less efficient, are sufficiently protected by the GM price umbrella to make excessive profits also. In the interest of profits, engineering improvements are played down in favor of superficial styling changes. Unwanted cars are forced on dealers. Cars in the smaller size and price range, preferred by many consumers, are not offered. Safety improvements are not made because they will not sell. Pollution control equipment is added only when mandated by government fiat.

Only a little needs to be added to White's excellent portrayal of industry behavior. His analysis of profitability, which is based on the worldwide earnings of the companies, would have benefited from a perusal of Securities and Exchange Commission files. The companies report in somewhat more detail to the SEC than to their stockholders, making it possible to develop a fairly reliable picture of their United States profitability. Several points can be added to White's account of industry profits. One, Ford and Chrysler are much closer to GM in domestic profitability than is generally known. Two, over the years the automotive industry as a whole has been the most profitable in this country. Three, the North American operations of the industry are generally more profitable than its overseas operations, measured either by profit margin on sales or by rate of return on investment.

The higher profitability at home than abroad has greatly influenced the industry's marketing strategy on small cars. Where it can, the industry practices market segmentation, the isolation and exploitation of high-profit markets. White cites two cases of market segmentation but misses the most important one, the isolation of the North American market from the rest of the world. The industry spends a great deal of money on market research and has always been sharply aware, not only of the existence of a huge domestic market for a small inexpensive car, but also of the likelihood that if such a car with its lower margin of profit were offered in the domestic market, it would steal a significant number of sales from its more profitable bigger brothers. Hence the motive for isolating the North American market with its fat profit margins from the rest of the world, where economic conditions, government regulations, and competition. The two-market strategy that worked so well for many much less profitable small cars. Nor could a small car be built at

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home for export, because a company could hardly refuse to sell at home what it produced at home. Hence the incentive to develop overseas operations to serve overseas markets.

Market segmentation cannot succeed in the presence of effective competition. The two-market strategy that worked so well for many years collapsed in the late 1950s when the North American market was invaded by foreign makes. White chronicles the events but misses the main point. The domestic industry first ignored and then disparaged the foreign competition in the vain hope that it would fade away. Next, the domestic companies turned to importing small cars from their own overseas plants in order to recapture some of the lost profits without committing domestic facilities to small car production. Only after the loss of profits became too painful did they undertake to manufacture the domestic "compact," a car larger and more expensive than the imports but close enough in size and price to offer competition.

The compact car was successful in stemming the tide of imports. If it had not been, the industry was prepared to step in with a truly small car, one in the same size and price range as the imports. In 1962, Ford was within months of commencing production of the subcompact Cardinal. GM is believed to have had the subcompact Fisher in preparation. Neither car ever reached production. It is important to recognize that the cancellation of the subcompact projects was motivated not by uncertainty about the size of the small car market, as White seems to think, but by the industry's pleasant realization that, at that time, it needed to go no further than the compact car to squeeze most of the imports out of the market.

White's treatment of auto industry profitability and the risk factor deserves some comment. He explains away part of the industry's higher than average profits as due to higher than average risk. He defines risk in terms of fluctuations of annual rates of return around the long-term mean and expects to find a positive correlation between the degree of risk so defined and the long-term average rate of return on investment: "Presumably, risk adverse investors will demand a risk premium before investing their money in a risky industry. Such high risk industries should presumably earn higher returns than the average of all industries."

This line of reasoning raises several questions. Are riskless investments unaffected by the business cycle? Will investors demand

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and be able to get a better than breakeven bargain on risk—that is, above average profits in good years that more than balance below-average profits (or losses) in bad years? How do the demands of investors, who operate in the arena of the securities market with its current values and price/earnings ratios, get translated into the historical values which show on company balance sheets, and thereby affect the rate of return on "book" investment? Do risks that confront the individual producers necessarily apply to the aggregate of producers in an industry? Might not the investor see a negative correlation between risk and the rate of return on "book" investment? In the auto industry, the lowest risk producer earns the highest return and the highest risk producer the lowest.

The case for the auto industry's being an above-average investment risk is unconvincing. It could be argued that if anything the risk is below average. After all, there have been no unsettling technological advances to digest, such as the oxygen process in the steel industry.

These are only minor points which, if anything, reinforce White's conclusion that a great deal is wanting in the performance of an industry whose profit-maximizing goals cause it to deliver an unsafe product which pollutes the air, costs more than it should, and does not meet the needs of those it is supposed to serve. However, when White turns from diagnosis to prescription, when he leaves the realm of descriptive economics and turns to social commentary, the book falls down badly. He sees that "the task of public policy in such cases should be to change the environment, if possible, so that social and profit-maximizing goals are in accord." But his recommendations are unimaginative and unrealistic and offer little hope of getting the job done.

White becomes hamstrung by an excess of objectivity. For him, solutions must confer cash benefits which outweigh precisely measurable costs. Hard numbers are demanded where none exist: "The difficulty is that there have been no clear estimates about the real costs of air pollution, not to mention the specific costs of automotive air pollution." This leaves him in a quandary. He is convinced by laboratory findings of the harmful effects of air pollution but uncertain about the economic benefits to be gained by doing something about it. The question whether government measures to control pollution have been worthwhile is answered

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with a "probable" yes, but he is bothered that government remedies to date imply "a strong preference for moral over least-cost solutions."

His tools of economic analysis are ill suited to the task of finding solutions to social and moral problems. On safety, he develops estimates of the economic loss caused by auto accidents but concludes, "Again, one would like to be able to ask, what are the costs and expected benefits from a particular safety measure? Unfortunately there do not appear to be any clear answers." He recognizes the weakness of this approach, but has no other to suggest.

He prefers private over public solutions: "Though it is unlikely that any of the current government imposed mandatory safety standards will be discarded, perhaps better response by the insurance companies in the future will reduce the need for additional mandatory safety standards." Of recommendations for further action he has few. He would educate the public about the performance characteristics of particular cars and how they relate to accidents. He would have the insurance industry give preferential rates to safe cars.

Finally, he turns to the problem of concentration. In his view, "This concentrated oligopoly is going to behave and perform much as in the past, as long as it remains concentrated." He would like to break up General Motors into three separate companies and Ford and Chrysler into two each. The additional "centers of initiative" would lead to meaningful competition with its attendant benefits of lower prices, reasonable profits, faster technological change and greater responsiveness to consumer needs. But having proposed radical surgery he concludes that the operation cannot be performed: "In this author's opinion, current judicial interpretations of the antitrust laws would not support a suit to break up the auto companies." By recommending what he considers to be impossible, he winds up recommending nothing.

A book which started out so well deserves a better ending. For excellence in formulating the problems White deserves high praise, but his readers will have to look elsewhere for the solutions.

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Book Reviews

History of the Supreme Court of the United States, *published under the Oliver Wendell Holmes Devise, edited by Paul A. Freund.* Vol. I: Antecedents and Beginnings to 1801 *by Julius Goebel* Jr.; Vol. VI: Reconstruction and Reunion, 1864-88, Part One *by Charles Fairman. New York, Macmillan, 1971.—xxv, 864 pp. $30.00; xx, 1540 pp. $30.00.*

Thirty-six years after Mr. Justice Holmes bequeathed his residual estate to the United States and sixteen years after Congress established a committee to administer the bequest, the first fruits of the committee's principal project have appeared. This is a history of the Supreme Court which in eleven volumes will tell the tale until 1941. The lapse of time has been such that the committee should be planning a twelfth volume that will bring the story down to Earl Warren's appointment as chief justice in 1953.

The figures on the length of these two volumes give only an inadequate notion of what Professor Goebel, with his penchant for the archaic,[1] would doubtless call the "magnific" scale of this work. Professor Fairman's book, mainly devoted to nine years of the Court's history, weighs five and one-half pounds. Even those with the deepest interest in the Supreme Court must be a bit turned off by the prospect of having to plough through some 13,000 pages, to pay $330, and to add 55 pounds to their library shelves, in order to get up to 1941. It is impossible to avoid re-

[1] Thus he relishes referring to the courts as "the judicial," although, as his text demonstrates, the Founding Fathers were also quite used to calling them the judiciary.

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peating the familiar tale of the father who gave his small daughter an authoritative book about penguins and was later informed, "Daddy, that book tells me more about penguins than I ever wanted to know." One may be permitted to wonder what Holmes, with his love of the terse and the readable, would have thought about this gargantuan venture. Certainly, the investment of time and vision required to read such ponderous volumes should be redeemed by an abundance of new insights.

I

Professor Goebel's book affords no such redemption. The Supreme Court does not even come on the scene until page 552, and then, if readers will pardon the phrase, somewhat circuitously, in a description of the onerous duties of the justices in riding circuit. The Court's "appellate practice" is reached only at page 662 and the substance of its appellate decisions at page 722. To be sure, no one would assert that a history of the Supreme Court should treat that body as if it had sprung from the head of President Washington when he made the initial nominations on September 24, 1789. However, some better sense of proportion ought to have been displayed; the origins should be treated not as a *Ding an sich* but as illuminating what was to come. Despite desultory mention of the sources of the doctrine of judicial review, the contribution to an understanding of the Court made in the 195 pages which take us up to the Constitutional Convention is utterly negligible. Even when we come to the deliberations of that body, the ratification struggle, and the Judiciary Act of 1789, which fill the bulk of this volume, the author should have paid greater heed to his own admonition that "our inquiry limits us to the discussion of the judicial" (p. 307). We could wish also that he would not have felt it necessary to include such extended summaries of the familiar. Presumably almost anyone willing to devote this much time to the origins of the "judicial" has not only heard of the Federalist Papers but has read those (Nos. 78-82) which are devoted to it. Why even attempt a lengthy summary when, as the author so convincingly demonstrates, "It is impossible to convey by paraphrase and brief quotation the qualities which made *The Federalist* a great book . . . " (p. 320)?

An example of the failure of this volume to live up to what might have been expected in light of the author's scholarship and industry is the chapter on the Judiciary Act of 1789 (pp. 457-508). Is it really important for the student of the Supreme Court to know about the absence of any mention of a bill of exceptions or about the use of error to secure review of chancery decrees, normal procedures in Connecticut, Ellsworth's home state, but not elsewhere (p. 479), or that the provision for a new trial after judgment was another Connecticut aberration (p. 482)? Professor

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Goebel's diligence has unearthed some errors, none particularly consequential, in Charles Warren's account of the development of this great statute,[2] but he lacks the earlier historian's narrative verve. Warren managed to keep his readers truly concerned about whether or not the first Congress was going to establish inferior federal courts in other than admiralty cases. While one knew the story was going to have a happy ending, the drama of the struggle was maintained. In contrast to the enormous attention devoted to minutiae of procedure, when Professor Goebel comes to a matter that was to be of truly great moment, § 34, the rules of decision clause, he spends little over a page (pp. 502-03). If he takes a position on whether the change from Ellsworth's original draft demonstrates that *Swift* v *Tyson* was in error, as Charles Warren thought and Brandeis was to accept, or confirmed the correctness of that decision, as Professor Crosskey was to assert, I cannot discern it.[3] Indeed, Professor Crosskey's book, unhappily not completed, is never mentioned in the text so far as I could discover and certainly not in the extensive bibliography. While I am on Professor Goebel's side [4] in the Crosskey controversy, is it right that in this definitive history of the Supreme Court, students should not even hear of Crosskey's learned and provocative argument that the framers intended to give the Supreme Court much wider lawmaking power than it ever asserted? Again, although the author evidently believes the framers of the Constitution and of the first Judiciary Act intended that the Supreme Court should be able to declare an act of Congress unconstitutional, the discussion is singularly lacking in depth.[5]

On another plane, one would suppose that a book like this would tell us more about the human side of the Court in its early days. The horrors of circuit riding are described (pp. 553-69), although this was done long

[2] Charles Warren, "New Light on the History of the Federal Judiciary Act of 1789," *Harvard Law Review*, XXXVII (1923), 49.

[3] *Swift* v *Tyson*, 41 U.S. (16 Pet.) 1 (1842). See Warren, 81-88. See also *Erie R.R.* v *Tompkins*, 304 U.S. 64, 72-73 (1938); Professor Goebel does seem to agree with Brandeis's conclusion but does not explain why. See William Winslow Crosskey, *Politics and the Constitution* (Chicago, 1953), I, II, 626-28, 866-71; and H. J. Friendly, "In Praise of Erie—and of the New Federal Common Law," *New York University Law Review*, XXXIX (1964), 383, 389-90, reprinted in *Benchmarks* (1967), 155, 161-62.

[4] Julius Goebel Jr., "Ex Parte Clio," *Columbia Law Review*, LIV (1954), 450. See Friendly, 392-94, *Benchmarks*, 164-67, and the articles cited there.

[5] For differing points of view, contrast Edward Samuel Corwin, *The Doctrine of Judicial Review* (Princeton, 1914) and E. S. Corwin, *Court over Constitution* (Princeton, 1938), with Raoul Berger, *Congress* v. *the Supreme Court* (Cambridge, Mass., 1969), another pertinent work that is never mentioned. See the book review of the latter by Joseph Cooper in the *Harvard Law Review*, LXXXV (1972), 702.

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ago.[6] But where and how did the justices live when they were at the seat of government?[7] What did they eat? How much did they drink? How did they get along with each other? In what kind of physical surroundings did they work?[8]

We ultimately learn, in a rather lifeless fashion, that the early Court did decide *Chisholm* v *Georgia*, thereby provoking the Eleventh Amendment; *Ware* v *Hylton*, upholding prerevolutionary British debts; *Glass* v *The Sloop Betsey*, making what would seem the unsurprising decision that prize came within the grant of admiralty and maritime jurisdiction; *Hylton* v *United States*, sustaining the validity of a congressional act taxing carriages without apportionment; and *Calder* v *Bull*, holding that the *ex post facto* clause of the Constitution applied only to criminal matters.[9] And for most readers, that, one might say, is that.[10]

II

Professor Fairman's book is an altogether different kettle of fish. To be sure, it is too large a kettle. Although the author is certainly right in thinking that the Reconstruction Court cannot be understood apart from the contemporary Congresses and presidents, he sometimes falls into the error of writing a history of the United States rather than of the Supreme Court of the United States. While he protests that "where a matter is dwelt upon, its relevance will ultimately appear" (p. 253), the ultimate is sometimes too long in coming for one with this writer's limited memory span. Subject to this single caveat, a reviewer can only burst into "songs of praise." Professor Freund has it right when he characterizes the book as "rich and penetrating, the product of many years of single-minded scholarly dedication" (p. xvi). I will single out three subjects peculiarly

[6] See Felix Frankfurter and James M. Landis, *The Business of the Supreme Court* (New York, 1928), 17-30.

[7] Only Iredell chose to move to Philadelphia (p. 554).

[8] Contrast the interesting description of the Reconstruction Court by Professor Fairman (pp. 62-70).

[9] 2 U.S. (2 Dall.) 419 (1793), discussed at pp. 725-37; 3 U.S. (3 Dall.) 199 (1796), discussed at pp. 748-56; 3 U.S. (3 Dall.) 6 (1794), discussed at pp. 761-65; 3 U.S. (3 Dall.) 171 (1796), discussed at pp. 778-82; 3 U.S. (3 Dall.) 386 (1798), discussed at pp. 782-84.

[10] Professor Goebel mentions *United States* v *Yale Todd*, decided in 1794 and reported in a note to *United States* v *Ferreira*, 54 U.S. (13 How.) 40, 52 (1851), which Professor Fairman considers to have been the first case in which the Court declared an act of Congress unconstitutional (p. 52, nn. 83, 85), only for the point that costs were collected (p. 708). His references to *Hayburn's Case*, 2 U.S. (2 Dall.) 409 (1792), the precursor of *Yale Todd*, are similarly trivial.

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pertinent to the moment: Congress's withdrawal of the Court's jurisdiction to review in *Ex parte McCardle;* the municipal bond cases, of which the best known but by no means the worst-decided was *Gelpcke* v *Dubuque;* and the recent reconstruction of the Reconstruction Civil Rights Act of 1866 in *Jones* v *Alfred H. Mayer Co.*[11]

The case *Ex parte McCardle,* the facts of which the book tells in rich detail, resulted after McCardle had published a number of scurrilous editorials in the *Vicksburg Times,* violently attacking the general commanding the district, officers of the Freedmen's Bureau, and Reconstruction generally. When he was held to appear before a military commission, he applied to the federal circuit court for a writ of habeas corpus. Upon this being denied, he appealed to the Supreme Court under the Act of February 5, 1867, which had been designed to enlarge the scope of the writ and to clarify the right to appeal.[12] On February 17, 1868, the Court sustained its appellate jurisdiction, and the case was argued by distinguished counsel for four days in early March. On March 27, Congress, overriding a veto, repealed the 1867 statute insofar as it permitted appeal to the Supreme Court.[13] Meanwhile, the Court had deferred decision; it adjourned in April without making one. A year later, it dismissed the appeal for want of jurisdiction, but with a dictum that the repeal affected only appeals from circuit courts under the Act of 1867 and not "the jurisdiction that was previously exercised," a dictum that was shortly to be implemented in *Ex parte Yerger,*[14] and deprived the repealing statute of much effect except in *McCardle*'s case.

Professor Fairman's reaction (pp. 509-14) to what has been generally regarded as a sorry chapter in American legal history will surprise most readers. A nosecount of the justices—apparently quite easy to make because of the garrulousness of several—showed "a majority ready to disallow the Congressional purpose [of Reconstruction], quite aside from any calculation about a Chief Justice who seemed always to be making calculations of his own" (p. 509). The needless breadth of Justice Davis's opinion in *Ex parte Milligan* [15] (see pp. 224-29, 236-37), coupled with his close friendship with Jeremiah S. Black, one of McCardle's counsel,[16] gave strong reason to believe that the seemingly inevitable reversal in *McCardle* would not be limited to the precise issue at hand:

[11] 73 U.S. (6 Wall.) 318 (1868); 74 U.S. (7 Wall.) 506 (1869). 68 U.S. (1 Wall.) 175 (1863). 392 U.S. 409 (1968).

[12] 14 Stat. 385.

[13] 15 Stat. 44.

[14] 75 U.S. (8 Wall.) 85 (1869).

[15] 71 U.S. (4 Wall.) 2 (1866).

[16] The other was David Dudley Field, brother of Mr. Justice Field.

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> The truth may be put in this arresting proposition, where latter-day readers may look at it squarely: one must believe that if Congress had failed to bring the weight of its authority upon the ten [Confederate] States, as then organized, there would have been no Fourteenth Amendment. . . . Submission to the Court as the true voice of the Constitution presupposes an established confidence in the lofty disinterestedness of its members—something that at the time of *McCardle* the Court did not enjoy and did not deserve (pp. 509-10, 514).

In other words, *McCardle* had the potential of being a second *Dred Scott* case;[17] Congress saved the Court from another self-inflicted wound. Very likely so, yet this is heady wine.

The Court did nothing to enhance the country's confidence by the series of opinions in diversity cases upholding the validity of bonds issued by towns and counties to aid railroads, despite contrary state court holdings. It is hardly news that *Gelpcke* v *Dubuque*, with its intemperate reference to the state supreme court's overruling its earlier support of the bonds' validity,[18] was a decision which "for years afterward the Justices were at a loss to find any adequate justification . . . " (p. 937). What is news for most of us is that *Gelpcke*, for which there was strong moral and economic if not legal justification, was perhaps the *most* defensible of the Court's decisions in the municipal bond area. The Court—and here the author takes the story into Waite's chief justiceship—"went much further: enforcing bonds which the State court had held invalid without overruling any decision; construing State statutes—on the powers of municipal officers, on debt limits, on the privileges of railroad corporations, etc.,—contrary to the construction given them by the State courts . . . " (p. 919). In a private letter Mr. Justice Miller, a consistent dissenter, characterized the decisions as "a mania," "a farce whose result is invariably the same, namely to give more to those who have already" —usually Eastern or European speculators who had bought the bonds at heavy discounts—"and to take away from those who have little, the little that they have" (pp. 1068-69). Attempts to enforce the judgments, requiring the appointment of federal marshals to levy taxes and foreclose on the property of defaulting taxpayers, caused widespread disorders in the Middle West. Resistance ended not so much because of acceptance of the Court's decisions as because of the prosperity brought by the railroads—at least by those that were built (pp. 1008-09). Some of today's activist federal district judges could profit from reading these chapters.

[17] *Scott* v *Sandford*, 60 U.S. (19 How.) 393 (1857).

[18] "We shall never immolate truth, justice, and the law, because a State tribunal has erected the altar and decreed the sacrifice"; 68 U.S. (1 Wall.) at 205-06.

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Professor Fairman ends his devastating criticism of the portion of *Jones* v *Mayer* which interprets the Civil Rights Act of 1966 as outlawing private discrimination in the sale of property by reflecting on the propriety of his discussion. He concludes, rightly in my view: "Either one must bow to that determination, and revise accordingly what had been written about the 39th Congress, or else examine the opinion critically for what it might be worth as an interpretation of history" (p. 1257). Beyond this it is fitting that a history of the Supreme Court should deal with the Court's use of history.

Although an earlier critique, which I did not find cited here,[19] had demonstrated the invalidity of the Court's assertion that the legislative history supported its novel construction of the statute, Professor Fairman makes no bones about his belief that the history proves the opposite. Any attempt to summarize the subtleties of his analysis would not do it justice. A particularly telling point is the opinion's heavy reliance on general remarks in the debates about the ending of discrimination despite the fact that a broad clause on that subject which was in the bill as introduced in and passed by the Senate (p. 1172) and remained throughout the debates in the House, was stricken immediately before passage by that body, because, in the words of the reporter, "some gentlemen were apprehensive that the words we propose to strike out might give warrant for a latitudinarian construction not intended" (p. 1193).[20] But the real bite of the criticism cuts deeper. In its use of history the Court has failed to heed—perhaps has wanted not to heed—the great teaching of Maitland, encapsulated in McIlwain's admonition: "No single fault has been the source of so much bad history as the reading back of later and sharper distinctions into earlier periods where they have no place."[21] Errors in detail are inevitable and excusable. But the basic fault with the Court's use of history, in *Jones* v *Mayer* and elsewhere,[22] is, as Professor Fair-

[19] Gerhard Casper, *Jones* v. *Mayer:* Clio, Bemused and Confused Muse," *Supreme Court Review* (Chicago, 1968), 89.

[20] None of the briefs had pointed this out (p. 1220), but the relevant history had been relied on, only two years earlier, by the same justice who wrote the *Jones* opinion, *Georgia* v *Rachel*, 384 U.S. 780, 791 (1966).

[21] See, for example, the extracts from Maitland's introduction to the Memoranda de Parliamento and his lecture, "Why the History of English Law Was Not Written," reprinted in Robert Livingston Schuyler, ed., *Frederick William Maitland, Historian* (Berkeley, 1960), 74-75, 137-38, and the much quoted passage in F. W. Maitland, *Domesday Book and Beyond* (Cambridge, 1897), 356. Charles Howard McIlwain, *The American Revolution: A Constitutional Interpretation* (Ithaca, 1958), 64.

[22] See Alfred Kelly, "Clio and the Court: An Illicit Love Affair," *Supreme Court Review* (Chicago, 1965), 119.

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man says, that it "appears to have had no feeling for the truth of history, but only to have read it through the glass of the Court's own purpose" (p. 1258). The thoughts which the *Jones* majority ascribed to the legislators of 1866 had not yet occurred to the justices, including two who joined in the *Jones* result, who decided *Shelly* v *Kraemer* [23] in 1948.

A study of the procedural history of *Jones* v *Mayer* in the Supreme Court causes wonder whether there is any way to cure this. The case was argued on April 1 and 2 and decided on June 17, 1968. The briefs, even the government's brief as amicus, from which something better might have been expected, naturally brought out only the "quotable snippets" (p. 1237) believed to favor the position advocated. Presumably a vote was taken at the conference held on April 5. Assuming that, in a case of this importance, the justices would have engaged in a fair amount of study before the argument, this must have been a minute fraction of the "large demand on time" (p. 1257) that Professor Fairman acknowledges to have been required for his historical exegesis. Once the vote was taken, did the history lead to the conclusion or did the conclusion mold the history? Remember also that the opinions in this case were prepared at the busiest time in the Court's year. Between April 1 and June 17, 1968, the Court rendered some 141 opinions on 70 cases, with the author of *Jones* writing 12 and the author of the dissent 27. A large part of the burden of preparing the *Jones* opinions must necessarily have fallen on the law clerks, and it would be sheer accident if they had been trained in history rather than in some other discipline. Yet "law-office history,"[24] worked over initially by a skilled law clerk and then by a busy justice,[25] was here used, not simply to give an added unction to a decision applying one of the great and vague commands of the Constitution but to support a hitherto undiscerned meaning of a century-old statute in an area where the Congress of 1968 had already passed legislation wider in some respects, more limited in others, and with more effective provision for enforcement.[26] There was thus not even an argument *ex necessitate* for such misuse of history, assuming, for the sake of argument, that there could ever be one.

Is there a remedy? Could the Court spot in advance those few cases where history may truly be decisive and commission a trained historian, or historians, to prepare analyses, which would then be made available

[23] 334 U.S. 1 (1948); see also *Hurd* v *Hodge*, 334 U.S. 24 (1948).

[24] See Kelly, 156.

[25] One cannot but wonder how deeply and objectively the justices who did not write opinions delved into the historical materials.

[26] Civil Rights Act of 1968, Pub. L. No. 90-284, tit VIII §§ 804-06, 82 Stat. 83-84, 42 U.S.C. §§ 3604-06 (1970).

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for criticism by counsel? Could the Court have a historian in residence? At the very least why could not a decision in a case like *Jones* have been put over to the fall, so that the justices and their clerks could undertake dispassionate research? Some such measures should be undertaken if the Court is to avoid such debacles as the historical portions of *Jones* v *Alfred H. Mayer Co.*

One would like to dwell on the masterly treatment of the Legal Tender Cases (pp. 677-775),[27] with its demonstration that the real vice was not Grant's "packing" the Court for a reargument but Chase's forcing the initial adverse decision while he still had the vote of Grier who had obviously become incapable of performing his duties; the illuminating discussion of the privileges and immunities clause of the Fourteenth Amendment and the Slaughterhouse Cases (pp. 1301-63);[28] what now seems the astonishing opinion of Mr. Justice Bradley in *Bradwell* v *Illinois*[29] sustaining Illinois's refusal to admit women to the practice of law; and the author's appraisal of Chase as a poor chief justice (pp. 1458, 1474-77). In light of this lack of leadership, the recurring presidential aspirations of Chase and Davis, and the generally low calibre of most of its members,[30] it is remarkable that the Reconstruction Court came out as well as it did.

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Kennedy Justice *by Victor S. Navasky. New York, Atheneum, 1971.—xx, 482 pp. $10.00.*

Navasky has written a vivid account of the highlights of Robert Kennedy's tenure as attorney general. It is not a balanced account, since it is based largely on the opinions and impressions of Mr. Kennedy's associates and admirers, whose starry-eyed, and often mawkish, views Navasky evidently shares. A skillful reporter, he has gathered his material with great diligence and accuracy; but he has not subjected the ma-

[27] *Hepburn* v *Griswold*, 75 U.S. (8 Wall.) 603 (1870), overruled in *Knox* v *Lee*, 79 U.S. (12 Wall.) 457 (1871).

[28] 83 U.S. (16 Wall.) 36 (1873).

[29] 83 U.S. (16 Wall.) 130 (1873). Two particularly toothful morsels are Miller's characterization of Bradley as "a queer man" and Strong's remark, "If there is a principle on which a case can be decided that no one else has thought of it has for that reason a charm for him" (pp. 1072-73, n. 209).

[30] Only Miller could be counted among the great justices, with Bradley and Field close behind.

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terial to either objective or incisive analysis, and, at times, he remains surprisingly insensitive to the moral implications of what he describes.

According to Navasky, Robert Kennedy, with his charisma, with his drive, and with his brother as president, had a chance to be the "maximum" (the term is Mr. Navasky's) attorney general. His failure to soar to new heights was due to extraneous factors. His unusual double bond of loyalty, as a cabinet member and the president's brother, made him doubly zealous to do what was in the president's political interest even if it could not be completely reconciled with his own lofty morality. Some of his able, but unfortunately gutless, subordinates persuaded him that compromises were sometimes better than open clashes. Finally, he was thwarted by the bureaucratic tactics of J. Edgar Hoover and the FBI.

His brother appointed "Bobby" (as he was then known) because he had run the 1960 campaign and was an expert practitioner of ruthless politics. No one—not even JFK—pretended that Bobby was a competent, much less distinguished, lawyer. Much to the new attorney general's credit, he surrounded himself with able subordinates, so that the technical aspects of the Department were handled with skill and dignity —and with little interference from the attorney general.

Mr. Navasky does not keep his promise to throw light on the decision-making process in the Department of Justice, except that he tells us decisions were made in informal, gregarious meetings, some around the Kennedy swimming pool. The nearest he comes to giving us the promised insight, and perhaps the most valuable part of the book, is his description of the deliberations and the subtle thinking that shaped the government's arguments addressed to the Supreme Court. The decisions of the Department were moral choices, with important political implications, but the ingenuity with which these moral/political choices were disguised as legal argument is technically truly impressive, although it is surely doubtful whether the legal arguments substantially influenced the Court's decisions.

Robert Kennedy himself emphasized blunt force rather than the dignity and self restraint which his high office required. When steel companies displeased the president by raising prices in the wake of a strike settlement, it was the attorney general who ordered the FBI to descend upon steel company executives before dawn, ostensibly to interrogate them. The attorney general's extraordinary action plainly had nothing to do with law enforcement. It was calculated to intimidate those who were rash enough to act contrary to the president's wishes, and to make politically helpful headlines.

Space does not permit an adequate assessment of many aspects of prosecutions instituted by Mr. Kennedy. The simplistic desire to root out

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evil and reward virtue, the author to the contrary (p. 445), is a perilous standard for the conduct of a prosecutor, especially if the definition of evil is left to subjective intuition. Even Mr. Navasky must admit that some of the prosecutions had a certain Keystone Cops flavor, but there were much more sinister aspects which Mr. Navasky does not mention. Kennedy used extensive exploitation in the mass media of his prosecutions to prejudice defendants and prospective defendants. Fairness was consistently brushed aside for the sake of publicity and the pressures it unloosened. Robert Kennedy became the only attorney general to save a murderer (Valachi) from the death penalty, in exchange for lurid tales that were of no concrete benefit to law enforcement but provided a feast for the mass media.

Where Navasky can be said to be at all critical of Attorney General Kennedy, is where his hero was not sufficiently doctrinaire, moralistic, and extreme. He deplores the fact that the attorney general, misled by brilliant but lawyer-like subordinates, occasionally compromised with Southern political leaders to avoid open clashes. Fortunately, the attorney general had better sense than his servants, and more political guile. As long as Kennedy's liberal constituency and the blacks saw in him a champion of their cause and as long as Southern politicians were able to act as champions of white supremacy, the interests of both sides were adequately served by staged token events—without the necessity of riots.

J. Edgar Hoover is the villain of Navasky's book: he was not sufficiently cooperative in taking part in the widely ballyhooed coordinated "attack" on organized crime; he refused to allow the FBI to be used as the attorney general's private army to protect freedom riders; above all, he wielded enough power himself to make decisions independently of the attorney general, his nominal superior.

It is silly for Navasky to suggest that Hoover became enraged at Martin Luther King because the latter was awarded the Nobel Peace Prize, "an honor which had eluded Hoover despite his years of service on behalf of public tranquility" (p. 153). One wonders who—other than Mr. Navasky—ever thought of Mr. Hoover as a Nobel Prize candidate.

After he ceased to be attorney general, Mr. Kennedy had to espouse the views of his liberal constituency—to whom both wiretapping and electronic eavesdropping were anathema. The attempt to change the facts retroactively was not successful. It is undeniable that as attorney general, Kennedy himself authorized the tapping of Martin Luther King's telephone; the most he could do as a candidate, later, was to issue misleading, evasive, and pettyfogging statements.

When the electronic eavesdropping (bugging) issue came before the Supreme Court, Nicholas Katzenbach, Kennedy's successor and former

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deputy, informed Kennedy that he would admit that bugging had been practiced by the FBI for some years prior to, as well as during, the Kennedy administration. Mr. Kennedy wanted Katzenbach to represent to the Supreme Court that this was without Kennedy's knowledge. The solicitor general, (now Mr. Justice) Thurgood Marshall, after a careful study of the documentary evidence, came to the painful conclusion that Attorney General Kennedy had known about it, and Katzenbach refused to make the kind of statement to the Supreme Court that Kennedy desperately wanted. Kennedy never forgave Katzenbach for his refusal to lie.

Despite Mr. Navasky, it seems that the Constable of Camelot possessed neither the lofty morality nor the intellectual stature that should have been indispensable qualifications for his high office.

BRUNO SCHACHNER
New York City

The Kennedy Round in American Trade Policy: The Twilight of the GATT? *by John W. Evans. Cambridge, Mass., Harvard University Press, 1971.—xiii, 383 pp. $13.95.*

The events described by this book have not attracted all the attention they deserve. The campaign to revise the trade agreements program, culminating in passage of the Trade Expansion Act, was among the chief legislative successes of the Kennedy administration. It reversed abruptly, if temporarily, the drift toward protection in the United States, granting the president new bargaining power, sweeping away an accretion of restrictions on his ability to use it, and altering the terms on which injured industries could obtain relief. The campaign, moreover, was one means to a larger end—Atlantic partnership—espoused in response or reaction to the creation of the European Economic Community, to Britain's first abortive application for admission, and to continuous French attacks on the Atlantic heritage and, equally, the European future. The trade negotiations carried out thereafter—the Kennedy Round itself—lasted some four years, witnessed important departures from tradition in the techniques of tariff bargaining, and sought to deal with new issues—the implications of Europe's agricultural policy, the demand from less-developed countries for special treatment, and an emerging concern with the restrictive effects of nontariff barriers.

The author of this book is well qualified to deal with much of this history. He led U.S. delegations to many GATT meetings and served as assistant special representative for trade negotiations during part of the period on which he reports. His narrative is authoritative, comprehen-

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sive, and readable—surprisingly, given his intricate subject. Those who are new to the subject, however, may acquire more information than they need, without also learning the answers to their questions, while specialists are bound to notice major omissions. The novice, for example, should protest Evans's failure to offer an economic analysis of the long debate on tariff disparities. Under what circumstances, if any, do equiproportional cuts in high and low tariffs have different effects on the volume of trade? The expert, by contrast, should protest Evans's scanty account of the major ways in which the Trade Expansion Act changed the escape clause.

More generally, the book falls short of the author's own promise to his readers. Its first part, he says, is meant as "an introduction to the general subject of commercial policy," its second describes "the political and economic climate that inspired the Kennedy administration to seek . . . the Trade Expansion Act," the third and fourth deal with the negotiations, and the fifth examines "the prospects for future rounds and, indeed, for the future of the multilateral trading system" (pp. x-xii). But none of them lives up to expectation.

Evans's introduction outlines the history of trade policy in general, and the trade agreements program in particular, since the Second World War, stressing issues that loomed large in the Kennedy Round (e.g., reciprocity, agriculture, and nontariff barriers). Some such introduction is always required, but this one is an unsatisfactory straddle. It is more than a mere prologue, yet less than a self-contained introduction to the theory and practice of commercial policy. It is not as complete as earlier accounts—and neglects to cite some of the best [e.g., Don D. Humphrey's *American Imports* (1955)]. And while it is more nearly current chronologically, it is not more modern analytically. It does not describe adequately or use efficiently the theory of effective protection (which appears only in the context of relations with the less-developed countries). Its brief account of customs union theory makes misleading use of Viner's distinction between trade creation and diversion (which pertains to the effects on global welfare, not the division of gains and losses between members and outsiders).

There is one innovation—Evans's explanation of the preoccupation with reciprocity:

> It is here suggested that the existence of tariff bargaining is in itself a sufficient reason for the insistence on reciprocity; anticipation of a future need for negotiating power provides incentive enough for hard present bargaining. Thus, the cost a government incurs when it reduces or binds a tariff may be measured less by any possible disadvantages from increased imports than by the value it believes a negotiating partner would place on that action (pp. 32-33).

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This hypothesis helps to illuminate the bargaining process, but is far from adequate. Why do negotiators want to conserve bargaining power —if not to secure future reductions in other countries' tariffs, thereby to win larger export markets? The participants in tariff bargaining may be innocent of mercantilist fallacies, but have still to justify their conduct to a public which judges it by ancient standards. Furthermore, Evans does not use his own hypothesis to explain systematically the process of bargaining during the Kennedy Round. His own account of the debate in the United States and of the negotiations in Geneva stresses different reasons for the insistence on reciprocity—including the condition of the American balance of payments.

Evans's description of the negotiations is skillful and disciplined. He has organized issues and events into manageable clusters. Yet we rarely see the forest as a whole, even though the trees have been grouped into groves. There is, for example, an interesting discussion of the negotiations on aluminum, where each participant's position reflected its own role in an integrated industry dominated by multinational firms. Yet Evans declines to draw any large inference from this episode, even when he comes to expound on the importance of multinational firms for the future of trade and tariff negotiations. There is, again, much discussion of the EEC's agricultural policy. One looks in vain, however, for a statement of the outcome—please do not send us to read the World Wheat Agreement—or a forecast of the prospects for future accord in agricultural matters. Evans does touch on one intriguing aspect of the Kennedy Round—the special problems facing the EEC and, more importantly, those who bargained with it, because the Europeans' negotiating brief was itself the inflexible product of internal compromise. He discusses the French boycott of the EEC in 1965-66 and the EEC's own struggle over the price of wheat. A political scientist, however, might want to know more about the general problem of bargaining with a contentious coalition.

Admittedly, the author labors under a handicap. The bargaining in GATT is confidential, even after the fact, and Evans cannot enumerate offers made, modified, or withdrawn. We are treated to occasional quotations from the press and are allowed to infer that these may not be wrong, but Evans cannot show us all of the trees in any particular grove.

The last part of the book is somewhat detached from what goes before. The author's prognosis—which gives the book its subtitle—derives in the main from an inspection of trends in the world at large, not from lessons learned at the bargaining table. His conclusions, moreover, are open to dispute. Evans stresses the many departures from nondiscrimination which have come to mar the international landscape. One wonders,

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however, whether frequent lapses from nondiscrimination in a world with moderate trade barriers are not better than perfect equality in the denial of access to important markets. One wonders, in addition, whether the preferential treatment given less-developed countries is not better for them and the world at large than the further pursuit of industrialization by import substitution in the less-developed countries.

All in all, Evans offers less than he has promised. But discount the promise, to concentrate upon the product. This book can be read with profit by students of diplomacy and should be read with care by many economists.

PETER B. KENEN
Princeton University

The Lives of William Benton *by Sidney Hyman. Chicago, University of Chicago Press, 1969.—xviii, 625 pp. $10.00.*

Promises to Keep: My Years in Public Life, 1941-1969 *by Chester Bowles. New York, Harper & Row, 1971.—xii, 657 pp. $12.95.*

Presumably there is some truth in the proposition that some part of the United States' economic "success" is founded on supersalesmanship and advertising, however annoying these ubiquitous perversions may be. Although neither William Benton's biographer nor Chester Bowles, writing about himself, actually says that they both became disgusted with the advertising hustle, one is inclined to give them the benefit of doubt and assume that is why they both abandoned the booming Benton and Bowles advertising agency in its prime. The sad thing, however, is that Benton never really changed his style; he has gone on being the clever promoter of schemes and puffer of superficialities ever since. Bowles did not forget his promotional gambits when he turned to the OPA, to Connecticut politics, and to international affairs, but he has ideas, cares about values, saw through the nonsense of crying Communist wolf constantly. In short, the similarities between the two men are swamped by the fundamental divergences of their personalities and the paths each chose on leaving Madison Avenue.

To be sure, they came to advertising from different worlds. Benton came from a family living in genteel poverty in the midwest, a family in which the father was weak and a self-doubting failure and the mother a strong-willed, dominating woman. Sydney Hyman in writing about these early years when a very unusual relationship existed between the future senator and his mother is very chary about what he says. One wishes for a more probing assessment of the trunkful of letters between Benton and

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his mother that Benton provided for his chosen biographer. From Benton's early teens until his mother's death in his forty-second year, he appears to have written to her twice a week, accepting her advice and ever seeking (but never getting) her approval for practically everything he undertook. (As a matter of fact, one would never know that Sigmund Freud ever lived from any direct evidence in either of these books.)

Bowles came from a wealthy New England family and like Benton went to Yale. (It apparently made little impact on either of them.) Unlike Benton, Bowles wanted to get into public service, but for various reasons was sidetracked into business. When he left advertising, his political career (and political education) began. In the next thirty years he devoted himself to politics, achieving varying degrees of success, but never reaching as high as his ambition propelled him. He can, however, look back with considerable pleasure to achievements and to insights that escaped others. In the Kennedy administration he was consistently annoying and got his head chopped off for it. When after less than a year as a New Frontiersman he was sacked from the office of under-secretary of state, many observers thought it had been the doing of the foreign service careerists; in fact it was his perceptiveness about the morass that the nation was moving into that made him troublesome to those wonderful folks who gave us Vietnam. Abjuring "I told you so's" or recriminations about those who did him in, Bowles quotes his memos to Secretary of State Dean Rusk and President John Kennedy against the Bay of Pigs fiasco, the increasing involvement in Vietnam, and our China policy, among other things. When the Pentagon Papers came out some said this was hindsight criticism—that such criticisms had not been made at the time of the decisions. But Chester Bowles had, much to his credit.

If Benton ever had such insights, they are hard to recall. He has promoted the *Encyclopedia Britannica* as if it were toothpaste, has dominated it personally and used it in his vast schemes of self-advertisement. So eager for fame and success is Benton that he tries to buy it by putting famous people on his payroll, presumably so that fame by association will envelop him. Thus he put both Adlai Stevenson and Hubert Humphrey on the *Britannica* payroll, paying well and expecting little from them. Hyman reports that Benton offered Stevenson an even better position with the *Britannica* operation just at the time of Stevenson's death—a fancy salary and the presumption that no more than 40 per cent of Stevenson's time would be devoted to *Britannica* business.

Even when Benton undertook the one thing that might get him a niche in the liberals' hall of heroes—his early attack on Senator Joseph McCarthy—one has the suspicion that he did it to dramatize himself, hoping to stir up sufficient controversy to attract the votes needed for his re-

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election. Others who were appalled at McCarthy lacked the guts to counterattack and counseled Benton not to, but he waded in and fought gamely. Indeed the possibility that the attack was a bit of self-promotion is suggested by the fact that McCarthy himself apparently understood that was what Benton was doing, and on one occasion told Benton, after an attack on McCarthy by Benton, "By God, Bill, you did marvelously today. I didn't think you could possibly be that good. I was certainly impressed." Perhaps it was a case of one faker recognizing another.

DUANE LOCKARD
Princeton University

Threshold to American Internationalism: Essays on the Foreign Policy of William McKinley *edited by Paolo E. Coletta. New York, Exposition Press, 1970.—334 pp. $8.00.*

The idea of a book on President William McKinley's foreign policy is excellent, but the essays in this volume are of uneven quality, and the promising theme of a threshold of internationalism is neither carried out nor assessed systematically.

The best chapter is clearly that by David Healy, whose broad-ranging study of McKinley as commander-in-chief should become a standard reference. Healy effectively analyzes the decrepit military establishment before 1898, the process and meaning of preparedness, and the bases of military strategy. He also measures the president's grasp of events, good judgment, and skill in political and military consensus against his tardiness in war preparations and occasional poor appointments. Healy's important conclusion is that McKinley became very knowledgeable about military matters, comprehended the uses of power, and was the first modern commander-in-chief.

Charles Campbell's chapter on Anglo-American relations is of interest because of his vigorous contention that rapprochement was based largely on wartime mood and was neither substantial nor lasting. Whitney Perkins, despite his neglect of a few recent studies, carefully assesses McKinley's colonial policies. The American belief in freedom, Congress's liberal inclinations, and McKinley's generous views undercut imperialism, argues Perkins, who concludes that McKinley responded remarkably well to an immense challenge.

George Pope Atkins's very brief essay on Latin American policy suffers from a naive analysis of party alignments and loose references to imperialism, but he usefully reminds us of McKinley's lack of interest in the Monroe Doctrine and his espousal of pan-Americanism. William

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R. Braisted, who writes on China policy, incorrectly claims that the public applauded the first Open Door notes and that the second notes were politically motivated. He exaggerates the importance of Rockhill and Hippisley and does not see, as Campbell does, the difference between collaboration with Great Britain and concerted action with all the powers. He brings out accurately McKinley's economic views, the role of Alvey Adee, aspects of the Boxer negotiations and withdrawal, and the important theme of "concurrence."

Julius W. Pratt's readable essay on the coming of war is balanced and traditional. He dismisses economic factors, treats Spain's views sensibly, and utilizes some recent literature though he has missed other items. His treatment of the sinking of the *Maine* is first-rate. By comparison, Paolo Coletta's chapters are disappointing, despite some insights and useful details on the beginnings of peace negotiations. The editor's essays are muddy, sometimes contradictory, and occasionally insubstantial. But he deserves credit for editing the volume.

PAUL S. HOLBO
University of Oregon

Free Soil, Free Labor, Free Men: The Ideology of the Republican Party Before the Civil War *by Eric Foner. New York, Oxford University Press, 1970.—xii, 353 pp. $8.50.*

Eric Foner's study of Republican ideology in the 1850s is the most significant contribution to the historiography of the causes of the Civil War to appear in several years. Along with Eugene D. Genovese's *The Political Economy of Slavery* (1965) it lays the ground work for a new version of the irrepressible-conflict thesis. The validity of Foner's interpretation hinges upon the importance of ideology in determining political behavior in the 1850s. Recent studies by political scientists and historians such as Walter Dean Burnham, Lee Benson, Samuel P. Hayes, and Paul Kleppner have minimized the role of issues and ideology and have emphasized the importance of ethnic, religious, and cultural factors in shaping American voting patterns, especially in the nineteenth century. Foner acknowledges the significance of ethno-cultural elements in the 1850s, particularly in the nativist and temperance movements of the decade, but argues persuasively that by 1856 the slavery issue had subsumed all others and that the Republican free-labor ideology had become the most powerful political force in the North. The 1850s, Foner believes, was the decade of the most pronounced "ideological focus" in American political history.

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Foner defines ideology as "the system of beliefs, values, fears, prejudices, reflexes, and commitments" held by an individual, class, party, or section. It provides the framework for "the way in which a group perceives itself and its values in relation to the society as a whole" (pp. 4-5). The Republican free-labor ideology was compounded of an amalgam of the Protestant work ethic and a belief in an open society with unlimited opportunities for social mobility. Its concept of labor embraced all productive work, including that of the farmer and entrepreneur as well as the artisan. In a fluid, expanding capitalist society a man might start as a wage laborer but if he worked hard, saved money, and seized the abundant opportunities available he could buy a farm or start a business of his own. The government's function was to maintain equality of opportunity for all and special privileges for none. Within this system, the success or failure of each individual depended entirely on his own exertions and abilities. This ideology appealed most strongly to the landowning farmers, independent artisans, small businessmen, and middle-class elements generally in rural and small-town Protestant communities in the North, and it was in precisely these areas that the Republican party received its greatest support. After the Civil War, the free-labor ideology evolved into Social Darwinism and the Horatio Alger myth; even in the 1850s, with the growth of the factory system and a permanent wage-labor class, it was obsolescent but it still bore enough relation to reality and a strong enough relation to Northern beliefs to become the dominant Northern ideology.

When Republicans looked to the South, they saw a society that appeared to violate all the values of the free-labor ideology, a society with a rigid, closed class structure dominated by an idle aristocracy, in which labor was degraded and opportunities for upward mobility were denied to four million blacks and millions of poor whites. Antithetical value systems in themselves would not necessarily have generated sectional conflict, but antislavery leaders perceived a slave-power conspiracy to control the federal government and expand slave society into the territories. The free-labor ideology, anticipating Frederick Jackson Turner, posited the necessity for continuous migration of free white workers and farmers to the territories in order to preserve opportunities for upward mobility in the North. The clash over the issue of slavery in the territories, therefore, was not merely symbolic of sectional differences; it lay at the root of these differences. Each side considered expansion of its own system necessary for survival; each perceived expansion of the other as a threat to survival. By 1860 many on both sides were convinced "that North and South represented two social systems whose values, interests,

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and future prospects were in sharp, perhaps mortal, conflict with one another" (p. 310). Foner considers Southern secession to have been a rational, logical response to the Republican victory in 1860, since leading Republicans had made no secret of their desire to put slavery on the path to ultimate extinction. Foner's interpretation dovetails nicely with Genovese's analysis of the ideological roots of Southern secession in *The Political Economy of Slavery*. Together the two books constitute a new and more sophisticated version of the irrepressible-conflict interpretation of the Civil War.

In its attitude toward the Negro, the free-labor ideology contained a "crucial ambiguity" apparent at the time and a serious shortcoming that only later became evident. While opposed to slavery and its denigration of black labor, many Republicans at the same time insisted that their program was designed primarily to benefit the free white laborer demeaned by slave competition. Their concern with slavery in the territories was mainly to keep blacks out of them rather than to strike a blow against bondage. Some Republicans insisted that theirs was the true "white man's party." Several recent studies have emphasized the strain of racism in the Republican party. Foner concedes that this strain existed, but he points out that, on the other hand, many Republicans did support equal civil rights for Northern blacks and were consistent in their application of the free-labor ideology to both races, sometimes at considerable political risk. Despite its ambivalence toward the Negro, the Republican party as a whole was in advance of Northern public opinion on this issue. But even pro-Negro Republicans subscribed to that part of the free-labor ideology which the failure of Reconstruction revealed as its major flaw, the "assumption that the major responsibility for a person's success or failure rested with himself, not society" (p. 262). Handicapped by slavery, racism, and discrimination, blacks could not enter the "race of life" with an even start, yet the self-help element of the free-labor ideology persisted long after the end of slavery and became the core of Booker T. Washington's racial strategy; its legacy lingers yet today.

One might wish that Foner had presented fuller and more precise voting data to buttress his generalizations about the sources and distribution of party and factional strength, but aside from this defect, his study is a major contribution to the historiography of the Civil War and a model case-study of the relation between ideology and political behavior. It deserves a wide readership among political scientists as well as historians.

JAMES M. MCPHERSON
Princeton University

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Joshua R. Giddings and the Tactics of Radical Politics *by James Brewer Stewart. Cleveland, The Press of Case Western Reserve University, 1970.—xiv, 318 pp. $8.50.*

The career of Joshua R. Giddings, who represented Ohio's Western Reserve in Congress from 1838 to 1858 and helped organize the Free Soil and Republican parties, has long been neglected, even by historians of the Civil War period. Thanks to James B. Stewart's well written biography, Giddings has now been rescued from obscurity. Drawing on a wide variety of sources, but relying primarily on the Giddings Papers in the Ohio Historical Society and on the files of Giddings's newspaper, the Ashtabula *Sentinel*, Stewart not only draws a convincing portrait of Giddings's career and character, but also raises some important questions about the role of radical politicians in the antislavery crusade.

Giddings is perhaps best known for his dramatic walkout at the 1860 Republican national convention, which forced the reluctant delegates to include in their platform a passage from the Declaration of Independence. But his most important contribution to the antislavery cause came twenty years earlier, when he took the lead in organizing the lonely band of congressmen who successfully attacked the major parties' conspiracy of silence on the slavery issue and forced slavery into the center of the political arena. Perhaps understandably, the towering figure of John Quincy Adams has obscured Giddings's role in this fight, but Stewart shows that Giddings was not only more of an abolitionist than Adams, but a better day-to-day organizer as well. Moreover, Giddings's reelection in 1842 after his censure by the House demonstrated that the Whig party would not be able to silence its antislavery wing and thus, as Gilbert Barnes pointed out many years ago, it was a major turning point in antislavery politics.

A politician and a moral reformer, Giddings combined two roles which abolitionists like Garrison believed were incompatible. Stewart, while sympathetic to his subject, does not attempt to conceal the inconsistencies which Giddings's position inevitably created. On several occasions, such as when he campaigned for Henry Clay and insisted that the Whigs were really an antislavery organization, Giddings was guilty, Stewart says, of self-deception.

As antislavery politics expanded its base of popular support in the 1850s, moralists like Giddings were inevitably pushed aside by such nimble political practitioners as Salmon P. Chase. Viewed through the lens of Giddings, the early Republican party seemed quite conservative, and perhaps Stewart at times underestimates the radical thrust of the

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Republican program and ideology. This, however, is only a minor problem: the book is a welcome addition to the growing historical literature on the antislavery movement.

ERIC FONER
Columbia University

Alexander Hamilton and the British Orientation of American Foreign Policy, 1783-1803 *by Helene Johnson Looze. The Hague, Mouton & Co., 1969.—132 pp. 21 Guilder.*

The Jay Treaty: Political Battleground of the Founding Fathers *by Jerald A. Combs. Berkeley, University of California Press, 1970.—xi, 254 pp. $7.75.*

These two books on the conduct of foreign relations during the early national history of the United States reveal both the flourishing state of current American scholarship and its extraordinarily uneven quality. Professor Looze's monograph is a routine, poorly written, academic exercise, its only claim to originality being its reorganization of familiar information; Professor Combs's book is a model of historical scholarship that offers a new way of looking at established facts and interpretations.

A major flaw of Professor Looze's research is her failure to utilize the abundant harvest of Hamiltonian scholarship produced over the past quarter of a century. The latest publication date of any book cited is 1961, and to that work there is a single reference in chapter I; other references, with two or three exceptions, are to works published before World War II. Only in a book based primarily on manuscript sources would such remissness be acceptable to other scholars. Professor Looze anticipates the fundamental question of any critic by asking: "Of what value has this study been, we might ask?" The answer, startling to one who believed that it has been generally the case for generations, is "Its major interest has been to place Alexander Hamilton in the respected position of a diplomatic statesman in American history, which he well deserves." In brief, the best that can be said of this book is that it affords a careful summary of Hamilton's reaction to successive episodes and crises of Anglo-American relations from 1783-1803.

Professor Combs's book deals with much the same period, but centers on the treaty that John Jay negotiated with England in 1794, indisputably the single most important and controversial treaty in this

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country's early national history. For almost half a century Samuel Flagg Bemis's *Jay's Treaty*—a detailed scholarly account of the treaty's background, negotiation, and significance—has been almost universally accepted as the definitive study of the subject. Why is it now in need of revision? "*Jay's Treaty* is an excellent work," Professor Combs explains, "but there are significant areas where Bemis is either incomplete, ambiguous or mistaken. . . . The present study seeks to resolve this ambiguity, and to supplement his treatment. . . ." In his attempt to achieve this ambitious goal, Combs takes a tack quite different from that of his distinguished predecessor. A comparison of the two is instructive, revealing as it does an important shift over the past fifty years in the approach to the study of American diplomatic history. Just as Bemis took the traditional approach—a detailed narrative and analysis of diplomatic negotiations largely based on official records—so Combs emphasizes his interest "in the intellectual history of American diplomacy"—the uncovering of the basic ideas and presuppositions of the men who guided the diplomacy as well as its general ideological orientation.

The thesis of Combs's monograph is that both Federalist and Republican leaders sought to "balance goals with the power available" but disagreed on the relative power of the United States and Great Britain. Who was right? Despite his sympathetic account of the beliefs on which Jefferson and Madison based their ideas on the proper conduct of American foreign policy and his obvious distaste for Hamilton's proposals, he concludes that the policies advocated by the Federalists were the more preceptive and constructive. This legerdemain is accompanied by asserting that "All in all, it seems that Washington, Adams, and Jay," not Hamilton, "followed the proper policy." Such an assertion is possible, however, only because Professor Combs magnifies the differences between Hamilton and his fellow Federalists and glosses over the fact that the broad outlines of their policies, as well as their premises, were precisely the same. Nevertheless, Combs achieves his major goal by convincingly revising a significant number of Bemis's widely accepted interpretations—of the reasons why the British ministry was unwilling to make major concessions to the United States, of Hamilton's alleged undermining of Jay's bargaining position, and of the motivation underlying Republican opposition to the treaty. This alone is a remarkable accomplishment and enough to assure Professor Combs's book a permanent place in the growing library on early American foreign policy.

JACOB E. COOKE
Lafayette College

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American City Planning Since 1890 *by Mel Scott. Berkeley, University of California Press, 1969.—xxii, 745 pp. $17.50.*

City planning is an ancient activity, but a modern profession. It has alternately inspired and frustrated its practitioners since Hellenic architects labored over the structure of Greek cities 2,500 and more years ago. It also has quite a long history in America. As John W. Reps has pointed out, William Penn's plan for Philadelphia and Major Pierre Charles L'Enfant's vision of a monumental Washington are simply the most famous of scores of important plans formulated in the New World before 1800.

American City Planning Since 1890 is a history of the profession rather than the activity. Commissioned by the American Institute of Planners to commemorate the fiftieth anniversary of the founding of that organization in 1917, Mel Scott has produced a solid, comprehensive, and scholarly account of the problems and achievements of a remarkable group of farsighted individuals over the past three-quarters of a century. It has some of the trappings of "official" history, but the author fully acknowledges that the history of planning in this country is, at best, one of very mixed success, and he makes important personal judgments about controversial issues. The scope is broad and the coverage encyclopedic; the text runs to 650 pages, the illustrations exceed 100, and the excellent index consumes 25 pages. Professor Scott obviously worked indefatigably interviewing participants, mastering the secondary literature on planning, and tracking down conference papers and memorandums. Particularly skillful is his handling of the changing definition of planning from its once almost exclusive concern with the structural and visual aspects of the city to its present stance of broad social awareness. Important topics such as the city-beautiful movement, the Columbian Exposition, and Burnham's Plan of Chicago receive the attention they deserve, but Scott also shows how other people and events made imaginative contributions that are less well known. This is a volume to which almost anyone can turn with profit, and it will doubtless remain a standard reference for many years.

I use the word "reference" advisedly. This is not a book that most people would find easy to read. The chapters are inordinately long and considerable space is devoted to dull recitals of proposals and conference papers, most of them never really implemented, that were presented at the annual meetings of the organization. Perhaps because Scott writes from a "design" rather than a "social" perspective, his neighborhoods and cities never come alive. He accepts uncritically the activities of urban reformers and appears to be unfamiliar with recent work on that move-

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ment. Little attention is given to such organizations as the Port of New York Authority, which, while not technically planning agencies, sometimes exercise considerably more influence on regional development than planning groups. Social scientists will be disappointed to find no careful, statistical analysis of data; in fact, there is not a single table or chart in the entire volume. Instead, figures on migration patterns, legislative appropriations, density, employment, and population are scattered through the prose so that systematic comparisons are difficult. Finally, the author skirts some of the broader implications of his topic. At a time when the planning profession is faulted by militants on both sides, one looks in vain for an attempted answer to the question, so what? What difference did it make to residents of Cleveland that their city was more receptive to planning during World War II than other cities? Was the quality of life improved? Perhaps because he is writing for professionals who share his assumptions, Scott felt no need to tackle such questions.

It would be unfair, however, to conclude on a critical note. Mel Scott has written a well informed and thorough account of the development of a critical profession.

KENNETH T. JACKSON
Columbia University

The New York Police: Colonial Times to 1901 *by James F. Richardson. New York, Oxford University Press, 1970.—xii, 332 pp. $8.50.*

Until recently American urban historians have given but a passing nod to the cop on the beat. Richardson's book, which concentrates on problems of public order in the nineteenth century and how police and citizens coped with them, goes a long way toward overcoming this neglect.

What immediately strikes the reader is the similarity of past problems and responses to those of today. Nineteenth-century New Yorkers had their poverty, ethnic conflict, alcoholism (instead of drug addiction), and crime. Some people criticized the police for not being vigilant enough; others accused them of brutality. Richardson reveals how the early development of the police led directly to many of today's fundamental problems, despite increased police professionalization. His research raises the question of how policemen related to the law in a government supposed to be one of laws, not men. Would the policeman develop as an impartial agent of the law or as an individual above the law, a judge who delivers his verdict with club or gun on the street?

Several factors forced the policeman into the latter position. A rural,

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Protestant state legislature, prodded by urban patricians, sought to legislate morality for New York City's cosmopolitan population (half foreign-born in the mid-nineteenth century). The police were charged with enforcing laws against gambling, prostitution, and Sunday liquor sales, laws which went against their own and perhaps most of the community's morality. Retention of unenforceable vice legislation on the books opened the door to systematic corruption, which enabled the policeman to ignore the law for a price, the "payoff." Caught between the conflicting demands of moralistic reformers and local politicians who dominated the force during most of the nineteenth century, the policeman did not regard himself as an agent of the rule of law. Moreover, when a policeman was willing to enforce laws such as those against disorderly conduct, he could not count on support from the judicial system. Police complaints of judicial leniency go back to the earliest years of the force. Judges, part of the political machine, were lenient not because of liberal humanitarianism but because "shoulder-hitters" were useful at election time. Finally, mutual escalation of criminal and police violence led many "respectable" people to accept summary police judgment on the streets as the best means of coping with the "dangerous classes." By the later nineteenth century, many of the same people who criticized police nonenforcement of vice laws endorsed harsh treatment of ruffians, rioters, or strikers. The New York police were established by a society which feared standing armies and arbitrary power, but fear of crime led to increasing tolerance of police methods inconsistent with America's democratic ideals.

Richardson explains increasing police arbitrariness in two ways. First, he contends that abolition in 1857 of the requirement that policemen live in the ward in which they serve may have made them distant from the local community. This argument raises crucial questions: were the police really on good terms with the community in the early years? Since residency was not *prohibited* after 1857, did policemen continue to live where they served? What did the police represent to different local communities? As Richardson indicates, such questions are difficult to answer with available historical material, but they can lead to further digging. Richardson's second explanation of arbitarary behavior is William Westley's finding that policemen employ brutality to gain a hostile public's respect. However, this theory can be tested by comparison with situations other than New York. In London, policemen in the force's early years faced considerable hostility but did not become excessively violent. Factors other than public hostility may have accounted for differences in behavior between London and New York policemen. The New York experience raises the question of whether public hostility or public *support*

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encouraged police brutality.

Richardson is a pioneer in a largely unexplored field. This volume is highly recommended as an informative introduction to police history which uncovers New York City's social history, suggests the roots of present-day police behavior, and provides leads for further research.

WILBUR R. MILLER JR.
Princeton University

Europe and America: The Next Ten Years *by W. Randolph Burgess and James Robert Huntley. New York, Walker and Company, 1970.—232 pp. $8.50.*

Europe and America is based on a two-fold premise: that Russian expansionism still threatens the Atlantic community during the seventies and that the nations of the Atlantic community must continue to act jointly through instruments like the North Atlantic Treaty and NATO to ward off the dangers of the future. The premise that the cold war persists and that NATO is essential for the peace of Europe and the world is considered self-evident for all perceptive students of recent Soviet foreign policy.

If one accepts the premise, which is never argued, the rest of the book's propositions fall neatly into place. They constitute a well reasoned analysis of the human, economic, social, military, and political problems which the Atlantic community faces in retaining its unity in the seventies, along with suggestions for new ways to cope with these problems. If one disagrees with the main thesis that "the coherence of the Atlantic system is the *sine qua non* of the preservation of our civilization" (p. 150), then one finds the book myopic in its narrow focus on the preservation of the Atlantic system as the only strategy to cope with East-West tensions.

The book is divided into three parts. It begins with an analysis of the human problems faced by the Atlantic community. These include the revolt of the young and underprivileged, particularly from the Third World, the impatience with conventional solutions and slow-paced reforms, the growing totalitarianism among the discontented, and the waning enthusiasm and lagging sense of urgency for NATO and its activities.

Part II scrutinizes economic trends of the past and their implications for the future. There is unstinted praise for the steps taken by governments and corporations within the Atlantic community to spur international trade. But tough problems remain, such as a need for reduction

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in nontariff trade barriers, the inadequacy of money and credit facilities, the harmful discrepancies in tax and business laws, and a lack of unified transportation facilities. The ultimate goal recommended for the Atlantic community is free trade for all Atlantic countries and selected others and special privileges for underdeveloped nations.

Part III deals with the military and political future of the Atlantic area. Despite some evidence of the reduction of East-West tensions and some chinks in the solidarity of the Warsaw Pact countries, the authors consider early settlement of major cold war issues unlikely. Consequently, NATO's military defenses must be kept intact, including retention of U.S. forces in Europe. While NATO's nuclear shield seems adequate at present, most other aspects of NATO's defense system are in serious need of upgrading to match growing Soviet military might.

The political machinery for consultation and joint foreign policy decisionmaking, too, is creaking. Ultimately, the authors would like to see the Atlantic countries uniting in some kind of federal union dedicated to common defense and development. They appreciate the many obstacles to this goal, including lack of enthusiasm in the U.S. and European fear of U.S. dominance and skepticism toward a federal plan. Still, they hope that the youth of the Atlantic community, if properly educated and socialized, will take the initiative towards federation.

Atlantic federation may seem utopian to many readers, as may many other specific proposals for change advocated in the book. But the proposals cannot be shrugged off easily, considering the experiences of the authors and collaborators. W. Randolph Burgess has served as U.S. ambassador to NATO and OEEC and was instrumental in the creation of OECD. James R. Huntley has served as a specialist on Atlantic affairs in the U.S. Foreign Service. Moreover, the ideas presented in the book were scrutinized by a committee of experts, each bringing specialized knowledge to the task. Members were selected from the ranks of the Atlantic Council of the United States. The Council, which sponsored the book, is a private organization which devotes itself to the analysis of problems of the Atlantic community. Whatever the merits of the arguments and suggestions propounded in the book, they are well worth knowing as representative of the viewpoints of an influential group of people. The authorship and sponsorship of the book well nigh assure it wide consideration as one possible plan of action for the development of Atlantic policy in the seventies.

DORIS A. GRABER
University of Illinois at Chicago Circle

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Europe's Would-Be Polity: Patterns of Change in the European Community *by Leon N. Lindberg and Stuart A. Scheingold. Englewood Cliffs, Prentice-Hall, Inc., 1970.—vi, 314 pp. $7.95.*

In *Europe's Would-Be Polity,* Leon Lindberg and Stuart Scheingold have greatly deepened prevalent appraisals of the multiple paths of integration, nonintegration, and disintegration which lie before the European powers in the 1970s. Their systematic presentation of alternative models of system change, linked with concrete but brief expositions of the contradictory processes at work, combine theory with the study of Community practices in an enlightening way. The pessimistic assumptions regarding the study of Britain's possible entry were a reasonable projection at the time the book was written, and it is still too early to say whether these assumptions will prove optimistic or pessimistic. In every other respect, the volume presents important new insights, some of which may be novel even for politicians and Eurocrats participating in the process.

The succinct analysis of alternative models of system change is itself so condensed but so clear that it is impossible to summarize it in a brief review. The authors have had the happy thought of contrasting the "forward-linkage" processes in the Common Agricultural Policy with the failure to achieve any similar integration in the area of transportation policy. Similarly, the advances made in implementing the customs union stand out well, despite the numerous internal contradictions, with the decline of integration in the sector of coal, in line with the declining role of coal in the European and other advanced economies.

In the light of experience since 1958, the authors conclude that economic integration is working effectively where the alignment of national interests demands this. On the other hand, the apprehension that attempts at political integration might jeopardize economic advantages already or prospectively gained makes it more and more likely that the members of the European Community, including the possible new members, will be happy to settle for a degree of integration that will not call into question the veto power of the national interest and will certainly not place in jeopardy the national decisionmaking processes.

The happy term, "the permissive consensus," summarizes very well the wide range of opportunities which the European Community offers its members for integrating those economic activities where they see a common advantage and leaving undeveloped those opportunities seen less attractive to one or more members of the Community. What emerges

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from this rich and deeply informed examination of both theory and practice is a new kind of "equilibrium theory," no longer vested with the virtues or risks of "automatic spill over" and ever deepening integration, but rather offering the machinery, the negotiating techniques for adjusting national interests, and an increasingly experienced but cautious commission and professional staff. The mystique of the initiating role of the commission has largely evaporated. Its alternative role as a mediator is shown to be effective only if it avoids acting as an independent maker of policy, leading it to concentrate more on its role in providing studies, proposals, and mediating formulas.

Because of its massive role in the European and world economy, the European Community requires constant re-analysis, so that we may understand where it is going or leading. No longer viewed as a panacea, the Community is, nevertheless, a weighty factor in world politics, and the study by Lindberg and Scheingold performs yeoman service in flashing its searchlight of rigorous analysis down the different paths which lead the Community into the future.

PHILIP E. MOSELY
Columbia University

The Appeal of Fascism: A Study of Intellectuals and Fascism, 1919-1945 *by Alastair Hamilton. New York, The Macmillan Company, 1971.—xxiii, 312 pp. $7.95.*

This is a disappointing book. A serious comparative study of the different kinds of appeal fascism(s) held out to a diversity of individuals and groups would be welcome. Most accounts of the intellectual Left between the wars have turned on this. The intellectual Right, hardly acknowledged, has received less attention. Its love/hate relationship with the fascists, too radical for some, not radical enough for others, has received almost none. The ambiguity of the subject has gone unrecognized (and Mr. Hamilton must be praised for having recognized it). But scholars have found it difficult to admit that the subject exists, while intellectuals ("of the Left") have preferred formulas to analysis. When, in his foreword to the present book, Stephen Spender expresses interest in the author's "question whether Charles Maurras . . . can really be considered a Fascist," he reflects the common tendency to lump and label: Maurras (like Franco and Churchill?) is a fascist, much as Castro (like Attlee or Allende?) is a Communist.

An attempt to introduce some order in this confusion, establish benchmarks, formulate definitions however tentative, seek for the motives

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that made intelligent men sympathize with fascist movements, support or join them, examine their commitment, their suspension of criticism or disbelief, their reasons including those reasons that reason does not know, their search for or their abandonment of faith, the assertion, adjustment, or abdication of their values—such an attempt would be extremely useful. It has not yet been made.

The promise of the cover is not kept. Mr. Hamilton shows *that* "some men of good will—artists and intellectuals—chose Fascism as their political creed." He never shows *why* they did so, as claimed. Nor, for that matter, does he always show their good will, their choice of a political creed, or their credentials as intellectuals. The appeal of fascism is superficially described, never analyzed; the chronicle of events replaces their discussion. The connection of certain writers and artists with some fascist movement is marked and traced, its implications hardly examined. No flesh grows on the printed names, no motivation pulses behind their words or gestures, no personality inhabits these personalities. Talented, wrong-headed men—D'Annunzio, Malaparte, Céline, Drieu, or Pound—vivid, bright-hued, controversial, acerbic, are drained of color and, in the end, of sense. Quite an achievement. The more's the pity.

EUGEN WEBER
University of California, Los Angeles

Dynamics of Counterrevolution in Europe, 1870-1956: An Analytic Framework *by Arno J. Mayer. New York, Harper & Row, Inc., 1971.—173 pp. $7.00.*

Professor Mayer rates himself in the preface of his study as a confirmed leftist and adds further that "the research design for any such sensitive undertaking is bound to be biased." The intent of an author to make his position clear from the very start is certainly to his credit, although an admission of bias cannot serve as its justification. The bias to which Mayer accedes is in essence the evaluation of revolution as a means of human progress in social and intellectual terms. Men who germinate genuine revolutionary thought and action by violent change are truly original and progressive. The broad international movement which fights those changes is counterrevolutionary. It stands in juxtaposition to the values created by revolution, since it is exclusively concerned with the defense of its own selfish interests and the establishment which stands for them.

It follows that the social scientists who may rightly or wrongly per-

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ceive both, specific revolutions and counterrevolutions alike, as manifestations of totalitarianism are consciously or unconsciously—should probably say subconsciously—"craving for social, professional or financial advancement." While this reviewer shares Mayer's rejection of the simplistic equalization of disparate concepts, he dissents from an opinion which charges one side, at worst, with overeagerness in the service of a noble cause and the other with base and/or stupid motives.

The author might have recognized revolution and counterrevolution as movement and countermovement in strictly logical terms which obtain their leftist or rightist political and social content, as the case may be, exclusively from the setting of a specific historic situation. In doing so, Mayer could have fully maintained his political position and yet his study would not have been loaded with built-in bias. In failing to follow this line of thought he is, it is true, in large and partly in quite distinguished company from the Right to the Left. Yet even if one discusses the problem entirely in the author's content-loaded terms, insuperable difficulties remain. They pertain first to his extreme addiction to compartmentalization of concepts. Mayer's definition of revolution is highly complex, as are his distinctions between conservatives, reactionaries, and counterrevolutionaries. Seven types of counterrevolution are discussed. What binds them together is primarily the fact that, according to Mayer, counterrevolutions are generally led by a rabble which has lost its status within the establishment and tries to recover it by recourse to violence. Such violence is an end in itself to counterrevolution, whereas it serves merely as means to the ends of revolution. This may indeed well be true in many cases but untrue in others. Unfortunately Professor Mayer steadfastly refuses to elucidate his classification by references to actual cases. Furthermore, he does not offer any real explanation why the era from 1870 to 1956 is the object of his study, a fact that can only be understood by readers familiar with his theory of the international civil war as discussed in previous, more fortunate writings of his. Concepts neatly separated like butterflies in a collector's showcase are lined up in a political no-man's land. Contact with reality would reveal that any given revolutionary or counterrevolutionary situation would spill over into several of the author's specifications. The infinite variety of historical situations plays havoc with Professor Mayer's vain attempts to arrive at truly comprehensive definitions. Nevertheless, entirely within his own terms of reference he could have done considerably better had he not entirely ignored analysis of the impact of one of the most important and baffling problems: the time factor in the sequence of revolutions and counterrevolutions.

While this abstruse book, with its highly complex language and its

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lack of adaptability to real situations, does not adequately represent historical or for that matter political theory as judged from any political angle, it does not lack distinct values. In the first place—and this is not meant facetiously at all—Professor Mayer's honest and tightly reasoned attempt demonstrates to others how not to tackle the difficult problems of historical typology. Therewith he has rendered an important service to future students in the field. Secondly, the study does not lack keen logical observation and perception of political and social, though not psychological, phenomena. It does not descend to the Marxian oversimplification of old, and it rarely resorts to glib cliches. This is no mean achievement.

ROBERT A. KANN
Rutgers University

Latin American Peasant Movements *edited by Henry A. Landsberger. Ithaca, Cornell University Press, 1969.—xi, 476 pp. $12.50.*

Although lacking an explicit theoretical framework, the descriptive material brought together in this book should prove useful to all students of social change in Latin America. The nearly 500 pages of text comprise eight chapters on peasant movements in seven Latin American countries (two chapters are concerned with Peru). These are flanked by a lengthy introduction by the editor and a concluding piece on obstacles to peasant movements.

The eight case studies are, on the whole, the most satisfying parts of the book; their restricted concern with national or subnational units lends them a focused empiricism which is lacking in the first and last chapters.

The case studies are opened by John Duncan Powell's discussion of the Peasant Union movement of Venezuela. His argument, that the peasant movement grew hand in hand with the democratization of the whole Venezuelan nation, is convincingly documented. Robert A. White S.J., follows with an attempt at a "natural history" of the Mexican revolution, with Zapata as its central focus, and succeeds admirably.

Dwight B. Heath gives us a fascinating account of the formation of peasant syndicates among the Aymara Indians of Bolivia in the wake of that country's revolution of 1952. It is an especially illuminating case, for here the peasant movements formed *after* the government in power had decided deliberately to encourage and even demand such movements.

Henry A. Landsberger on the vineyard workers' strike (Chile), Julio

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Cotler and Felipe Portocarrero on peasant organizations in Peru, Neale J. Pearson on the Peasant Union movement (Guatemala), and Cynthia N. Hewitt on the Peasant Leagues (N.E. Brazil) all present a wealth of facts on their respective subjects. Yet their descriptions focus on personalities and ideology at the expense of socio-economic process, and all suffer from an absence of organizing principles by which to make clear the relevance of the facts presented.

Wesley Craig's fine discussion of the peasant movement in La Convencion Valley (Peru) is refreshing in its compactness; of all the case studies, his offers the most carefully reasoned *explanation* of a peasant movement and is especially recommended reading.

The introductory chapter by the editor is, at its best, exploratory and suggestive; at its worst it is simply confused. Attempting to maintain an open-minded eclecticism, Landsberger seems at times to want to deny any patterning at all in the occurrence of peasant movements. Each generalization is followed by a discussion of exceptions which are so weighted by the author as to emasculate almost totally the original generalizations.

One of the main flaws in Landsberger's approach is that he defines the term *peasant* broadly and vaguely as "a rural cultivator of low economic and political status." He does not wish unnecessarily to limit the domain of discourse and thereby exclude some highly relevant movements, such as strikes by rural wage earners. I agree with his goal of inclusiveness, but only if it is accompanied by an analytical framework sufficiently detailed to indicate which differential characteristics of varieties of "rural poor" are likely to have implications for understanding the origins, trajectories, and outcomes of peasant movements. This Landsberger does not do. His discussion floats nebulously over the whole range of rural activism, from Wat Tyler's revolt in England (1381), to the French Revolution, to northeastern Brazil (early 1960s), and so on, picking his examples casually, without ever stating why he feels he can draw valid comparisons from evidence of such wide spacio-temporal discontinuity.

Ernest Feder concludes the volume with a somewhat polemical article which usefully points to the whole range of social and economic forces which surround the peasantry and impede the growth of effective peasant organizations. He tends, however, to follow a kind of liberal conventional wisdom about downtrodden peasants which avoids asking why the institutions of traditional agrarian society ever existed at all, and why so many peasants have regarded them not just as inevitable, but also legitimate.

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The strength of this book, then, is in its case materials, and it should be approached with that in mind.

ALLEN W. JOHNSON
Columbia University

The Transformation of Political Culture in Cuba *by Richard R. Fagen. Stanford, Stanford University Press, 1969.—ix, 271 pp. $8.50.*

This book is an interesting attempt at interpreting the Cuban Revolution in terms of political socialization; that is, the inculcation of new political values, the "making of citizens" who are to be "new men," "Communist men": "The Cuban Revolution involves a massive attempt by the political elite to forge a new social order by forming and reforming men who will worship new images, behave in new ways, and relate to one another in the context of new institutions." The Cuban revolutionary leaders themselves are held to see their task to be reeducation of the masses: "revolution and education are the same thing," Fidel Castro himself has said. The mechanism for transforming the Cubans' political culture is directed participation in revolutionary activities and institutions. The author examines the Campaign Against Illiteracy of 1961, the Committees for the Defense of the Revolution (CDR—founded in 1960 and still in existence), and the Schools of Revolutionary Instruction (founded in 1960 and discontinued in 1967). The author does not make clear why he chose these three institutions, but the choice affects the conclusions. Both the Campaign Against Illiteracy and the CDRs are mass undertakings. The same would apply to the Federación de Mujeres Cubanas, for instance; its functions are noncontroversial, integrative over a wide social-status spectrum. But this would not be the case, for instance, with the Asociación Nacional de Agricultores Pequeños (ANAP), which was born as a peasant organization opposed to the middle-class farmers' organization, the Asociación de Colonos. It then worked for some years as a giant service cooperative, and it has nowadays the function of incorporating the peasants into the state sector in a slow program of collectivization through a mixture of cajoling and compulsion. To say that the function of ANAP is to educate peasants to become "new men" (in the context, proletarians) would sound like a joke. By focusing on the CDRs the author tends, in my view, to give an impression of larger institutionalized political participation than really exists. For instance, a study of the unions would lead one toward the conclusion that participation is discouraged; thus, such a momentous decision as the introduction

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of "work-norms," and therefore the institutionalization of piece-work payment in 1962 (a decision which went against the whole egalitarian ethos of later years), was taken by the ministry of labor with little open discussion in any of the political organizations. An examination of the important role of the Communist party might have led the author to doubt whether "participation" was the right word to use in describing the lack of involvement of the population in institutionalized political life; the party has yet to have its first congress.

On the other hand, the author tends, in my view, to give the leaders more than their due as sources of ideas. The phraseology and the slogans (such as "the new man") may be theirs, but the inspiration and the social base for such ideas are to be found in the working class. It is true that at times the leaders might have given the impression that they were engaged in an educational process, that their primary goal was "the transformation of Cuban man into revolutionary man," and that therefore age (youth) was more important than class (the workers). But there is little doubt that the leaders' decisions, and the evolution of the Cuban Revolution, cannot be understood excluding any reference to the socialist category of class struggle since the leaders themselves sometimes use it. In the summer of 1970, Fidel Castro made speeches emphasizing how the revolutionary leaders themselves had much to learn from the "socialist consciousness" of the masses, who were very equalitarian and would not tolerate privilege. Though the emphasis on the working-class origin of the political values of the revolution is new in Fidel Castro, the author should not have missed this theme so completely.

J. MARTINEZ-ALIER
St. Antony's College, Oxford

Haiti: The Politics of Squalor *by Robert I. Rotberg with Christopher K. Clague. Boston, Houghton Mifflin Company, A Twentieth Century Fund Study, 1971.—xvi, 456 pp. $10.00.*

It is a pity that the most important book on the Haitian political economy to appear since Professor Leyburn's *The Haitian People* in 1941 should be so contradictory and essentially unreadable. Both liabilities have to do with the fallacy of foundations assigning subjects to researchers or economists, regardless of whether the "author" of the resulting "book" (or non-book) can write, or has any empathy for the subject. In this case the contradictions abound. It is never clear whether the "squalor" in the title refers to the condition of the Haitian people, or to the dictatorship of François Duvalier on which Mr. Rotberg focuses when he is not docu-

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menting the squalor. The dedication—"For Toussaint, Christophe and other freedom-loving Haitians"—is equally ambiguous since Mr. Rotberg is at pains to show that the two masterful generals who emerged from the slave insurrection of 1791 attempted to fasten upon their liberated subjects dictatorships as absolute and unacceptable as slavery itself. No trace of sympathy for the great qualities of the Haitian people—their friendliness and generosity, their courage and humor, their genius for song, dance, poetry, and pictorial expression—is revealed, though a half-hearted attempt is made from time to time to account for their susceptibility to bad government through a psychoanalytic theory of the abrupt withdrawal of "ego-sustaining and ego-developing nurturance" when the child is weaned. But the style, an atrocious bombardment of jargon words like "parameters," "superordination," "infrastructure," "lissiparous," "inputs," "kleptocracy," and soggy latinisms like "the levelling of excrescences of potential dissidence," need only be revealed in one quoted sentence to indicate the barrier to communication:

> Furthermore, although the differences between grinding poverty (the minimum daily wage is 70 cents [U.S.], but this statutory requirement is still honored more in the breach than in fact, and many workers are employed only on a seasonal basis) and excessively grinding poverty can probably be measured per capita, once a man is inured to living on the most precarious of margins, increasing the thinness of the margin tends to produce no newly accentuated emotional response.

The pity is that buried in all this verbiage are the raw materials for several books. One book would be the analysis of the French colony and the Haitian governments that succeeded it in the nineteenth century. This Rotberg documents in greater detail than Leyburn, though with none of the clarity and felicitous style of the Yale professor. The very first post-revolutionary constitution, Toussaint's, concentrated all the powers of the legislatures, the military, and the Church in the ruler's hands, exactly as does Duvalier's today—even similarly naming the dictator governor-for-life with the power to name his successor. Toussaint, Christophe, and Dessalines could make Haiti produce; but once the viable plantations had been atomized into minifundia bye Pétion and Boyer in the next generation, every subsequent dictator (including Duvalier) was reduced to plundering the sickly state itself. When Boyer was intimidated by France into mortgaging the remnants of the prosperous colonial economy as payment of "debts" for the properties seized, the last possibility of developing a capitalist economy—even as primitive a one as Jamaica's or Barbados's—vanished.

A second book could comprise the sorry record of the United States' involvements in Haiti, both during the American marine occupation of

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1915-34 and subsequently when Duvalier was first vigorously supported and then isolated but not ousted. Rotberg's account of the occupation is superior to any other; but his research in the 1957-69 period depends almost wholly on secondary sources such as the sensationalized reportage of Bernard Diederich's and Al Burt's *Papa Doc*. Our ambassadors of that period were not interviewed, nor is there any explanation of why Juan Bosch was encouraged to invade Haiti and topple its (then) tottering dictator, only to be restrained at the zero hour.

Finally there is the book implicit in the account of Duvalier's personality and regime that runs inconclusively throughout the volume. No writer has come close to presenting so much evidence, all of it damning; yet Rotberg comes no closer than the others to explaining the enigma of Papa Doc's survival or proposing practical alternatives for Haiti's future. Why the American government failed to support the shortwave broadcasts in Creole of such a moderate exiles' group as Raymond Joseph's Haitian Coalition—instead of blocking them, leaving the airwaves entirely to Fidel Castro—is the greatest enigma of all. For until some such group as Joseph's is enabled to take over and receives massive American support, Haiti cannot even begin to rise above starvation and face its age-old problems.

SELDEN RODMAN
Oakland, New Jersey

The Middle Beat: A Correspondent's View of Mexico, Guatemala, and El Salvador *by Paul P. Kennedy, edited by Stanley R. Ross. New York, Teachers College Press, 1971.—xx, 236 pp. $8.50.*

Politics and Society in Mexico *by Martin C. Needler. Albuquerque, University of New Mexico Press, 1971.—xvii, 144 pp. $6.00.*

These two books are welcome additions to our literature, if for no other reason than their enlargement of the material currently available in English on a region of growing significance. While the progress of Mexican politics is of common concern to the two volumes, the differences between them—one written by a political scientist and the other by a journalist—are at least equally noteworthy.

In *Politics and Society in Mexico*, Martin C. Needler, a political scientist at the University of New Mexico, presents an essentially optimistic view of the recent course of Mexican political and economic development. Albeit belatedly, he embraces the thesis, pioneered by Leon V. Padgett some fifteen years ago, that a variant of representative democracy not

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only is possible but indeed actually operates within the one-party system of Mexico. Needler further accepts the doctrine advanced in the early 1960s by the Social Science Research Council's Committee on Comparative Politics that democratic or nondictatorial one-party systems are to be regarded as a fairly common phenomenon in underdeveloped areas. With respect to the problem of militarism, Needler associates himself with what has been written about the professionalization and the political neutralization, while in general ignoring the bureaucratization, of the armed forces of Mexico. He suggests two propositions in the matter of political corruption in the country. The first is that corruption has been diminishing in recent years, and the other is that what remains of it performs a positive function anyway. Both are interesting propositions; both need hard confirming or refuting research. The study ends on an all's-well-with-Mexico note, emphasizing economic progress and political democratization.

From 1954 to 1964, Paul P. Kennedy was a correspondent of the *New York Times* in Mexico and Central America. He died before his book was completed, and the manuscript was edited for publication by Stanley R. Ross of the University of Texas. The introduction to the work indicates that Kennedy had been a remarkably likable fellow, well known and loved by most of the presidents, politicians, editors, bartenders, cab drivers, and smugglers who worked at their respective trades in Mexico, Guatemala, and El Salvador during the 1950s and 1960s. The term "Middle America," used by some geographers and geologists to refer to Mexico, Central America, and the Caribbean area, is the source from which Kennedy derived "Middle Beat" as a newspaperman's designation for that region. Kennedy's book is essentially a personal account of his own adventures and observations, many of them fascinating, during the decade in which he covered Mexican, Guatemalan, and Salvadoran news.

Two observations should be made about these books considered together. One point has to do with the process of arriving at general propositions, the other with the bright-hued glasses through which the authors, political scientist as well as journalist, view the Middle American future. With respect to the first of these observations, Needler, for his part, adds some new and current data without altering essentially or contributing new insights into the general theoretical constructs developed in the 1950s and 1960s regarding political development and one-party systems in the developing areas. Thus, in Needler's hands these constructs come to be doctrines repeated uncritically as a species of academic party line rather than general propositions to be subjected to the tests of research. Kennedy had his generalizations, too, but the journalist *qua* journalist may well be entitled to evaluation via different criteria.

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Why should the social scientist wonder if Kennedy's notion of "Middle American society" is a valid cross-national concept when we read in *The Middle Beat* of the volatility common to the society, politics, and *geology* of the countries involved? It may well be that the journalist is entitled to this especial license. Surely Kennedy's impressions and insights are provocative if untested.

Secondly, we are presented here with two optimistic books. Needler integrates what social scientists have written about economic development, political modernization, and democratic one-party systems to give us a basically hopeful picture of Mexico's future, uncomplicated by a generation gap or growing disenchantment with the establishment of the Mexican Revolution. Kennedy tells ten years' worth of delightful and fascinating stories which suggest to him that Mexico, Guatemala, and El Salvador experienced wholesome and tangible progress while he covered that beat. Even Ross is worried about this, wondering if affairs might not have seemed more hopeful in 1964, when Kennedy bade a fond farewell to this assignment, than they did in 1971, when the book was finally published. In any case—and whatever the explanation—both volumes seem unrealistically optimistic and innocent of the worries, concerns, and misgivings of the 1970s.

GEORGE I. BLANKSTEN
Northwestern University

Chinese Intellectuals and the Revolution of 1911: The Birth of Modern Chinese Radicalism *by Michael Gasster. Seattle, University of Washington Press, 1969.—xiv, 288 pp. $9.50.*

Probably the most significant recent development in the Western study of twentieth-century China has been the discovery of the decade—or rather of the decade and one-half—which preceded the 1911 revolution. The significance of the revolution itself remains a matter of much contention, but recent study has made it abundantly clear that the birth of the modern Chinese intelligentsia is to be sought not in the May 4th movement of 1919 but somewhere within the period from 1890 to 1911. It is now clear that among the path breakers of the new intelligentsia were men whose early formation had taken place largely within the framework of "traditional" China, but who had finally burst out of the confines of Chinese cultural experience in their urgent search for new answers. It was during these years that some of the questions were posed which have dominated the entire subsequent history of modern China. It was during

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the period that there emerged within the small Chinese intelligentsia a remarkable openness to the whole gamut of modern Western social and political thought.

Mr. Gasster's book on the Chinese intellectuals before 1911 is one of the more significant of a growing body of monographs on this period. It is particularly valuable as an introduction to the thinking of those intellectuals who tended to call themselves revolutionaries. After this book it will no longer be possible to dispose of this group by a few references to Sun Yat-sen's Three People's Principles and to "republican" ideas. Mr. Gasster introduces us to all the complex and tangled discussions of the newly imported notions of democracy, socialism, anarchism, nationalism, science, etc. which both excited and perplexed them. A particularly valuable contribution is Mr. Gasster's chapter on the idiosyncratic "traditionalist" revolutionary, Chang Ping-lin, a figure who illustrates the difficulty of treating modern Chinese intellectual and political history in terms of a simple conflict between "tradition" and "modernity."

Mr. Gasster's book is also informed with a central thesis concerning the ascendancy of radicalism as a dominant state of mind within the Chinese intelligentsia. Beginning with the first decade of this century, the tendency to search for total, all-embracing "revolutionary" solutions of China's problems persisted down to the rise of the People's Republic. Indeed, in the person of Chairman Mao it has continued on into the present.

It seems to me that there is much truth in this thesis although it may not be as illuminating a truth as the author implies. The yearning for total solutions is not the same as the discovery of total solutions. In fact, the radicals in China have been no freer than the nonradicals from all the dilemmas and puzzles created by the pursuit of such vague and general goals as democracy, socialism, nationalism, and modernization. The question of the relationship among these goals has not been resolved in the Western heartland of modernity itself, while in China their pursuit has been complicated by all the specificities of Chinese history and Chinese culture, and the end is not yet in sight.

BENJAMIN I. SCHWARTZ
Harvard University

The Kuomintang-Communist Struggle in China, 1922-1949 *by Chung-Gi Kwei. The Hague, Martinus Nijhoff, 1970.—viii, 131 pp. $6.50.*

The last paragraph in this book, written by a man who is a professor at Soochow University in Taiwan, and who has been from the early twenties

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a leading member of Kuomintang, reads:

> The author's own observations led him to believe that if we cannot destroy armed Communists, we shall be destroyed by them, and that if we cannot take effective precautionary measures to cope with unarmed Communists, we shall be eroded, dismembered, and finally destroyed by them. This may be said to be an unrelenting law which no one can ignore with impunity.

There have been hundreds of books written with a similar conclusion, either implied or stated. But none has been written with fewer accurate facts and more deliberate omissions than Professor Kwei's book.

Much of his material is derived from printed documents published in Taiwan that are not generally referred to as sources by Western China experts. In addition, many conclusions are based on what some important individual "personally told the author." Some conclusions are based on a variety of reminiscences. For example, dealing with the period of the early 1920s, Professor Kwei writes:

> A reported interview between Soviet Dictator Stalin and a Chinese is illustrative and illuminating. Stalin, according to the report, was said to have asked his interviewer, "Do you think that the Chinese bankers and industrialists in Shanghai would contribute money for Chen Tu-hsiu, Chairman of the Central Executive Committee of the Chinese Communist Party, to establish a daily paper?" (p. 17).

Stalin could not have asked that question, even if this report is accurate, in just that way. And Stalin was far from being a "dictator" in the early 1920s.

Kwei's book is full of errors. M.N. Roy was not a "Trotskyite," was not tried in Moscow and given a "five year sentence." Chiang Kai-shek's kidnapping is not accurately described, nor is the Canton Commune or the Long March.

Regardless of the validity of the final conclusion of this book, it is factually almost worthless.

EARL BROWDER
Princeton, New Jersey

Toward Restoration: The Growth of Political Consciousness in Tokugawa Japan *by H. D. Harootunian. Berkeley, University of California Press, 1970.—xviii, 421 pp. $10.00.*

In this stimulating and highly original study, Professor Harootunian discusses the early nineteenth-century debate in Japan over how to respond to the dual problem of "disorder from within" and "threat from without." The main theme of the book is the breakdown of Confucianized

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conceptions of government under pressure from an insistent and recalcitrant West which abided by none of the conceptions Confucianists held dear. Professor Harootunian argues that contact with the West shattered the idea that the difficulties of the realm could be overcome by devoted observance of eternal Confucian verities, which justified institutional arrangements but did not explain them. Faced with the possible loss of national independence, some men began to realize that they had to come to grips with existing institutional realities rather than to retreat to facile moral pieties about the "kingly way." The new mentality was summed up rather neatly by Yoshida Shōin's observation to Kusaka Genzui in 1856 that "the realm is not made by heaven and earth, it is made by men. Our approach to affairs must always be based on our assessment of what is needed" (p. 218). This attitude prompted a recognition that the only feasible path of action for Japan was rearrangement of her institutions to meet the needs of defense against the West. The result of the erosion of the belief that politics and virtue were synonymous, Harootunian suggests, was the growth of "political consciousness" (a recognition that politics was synonymous with power) and the discovery of a new "political space" (a recognition that Japan was not simply a moral community but a territorial entity with definable interests and a unique historical identity).

The main flaw of the book is that it never really clarifies the question of what made possible this transformation in political mentality. Harootunian seems to suggest that a key role was played by "nativism," an alternative tradition of discourse which, despite its "irrational" origins, provided the basis for a more rational response to the West than the "rationalism" of Confucianism. But the reader is not always sure what is meant by nativism—sometimes it refers to *kokugaku*, sometimes to the myths of the imperial Shinto cult, and sometimes to a generalized conception of Japan's uniqueness. Further, although Harootunian frequently asserts the importance of nativism in shaping the new political consciousness, he is never very specific as to how. For example, in his discussion of the sources of Yoshida Shōin's thought, Harootunian dismisses Maruyama Masao's argument that Yoshida was moved by a "submerged tradition" of a pre-Confucian warrior ethic and suggests instead that Yoshida appealed rather to a more available nativism (here used in the sense of the notion of imperial uniqueness). But in the subsequent discussion, Harootunian does not develop the observation further, and, moreover, cites abundant evidence for the conclusion that however important "nativism" was in Yoshida's thought, he was equally inspired in his final years by a cult of action that predated both nativism and Confucianism.

The book has other flaws as well, many of them stylistic. It is overlong,

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marred occasionally by passages that are murky and frequently by those that are prolix. The author, no doubt, squeezed his original manuscript, but like Motoori's seed it could have been squeezed some more. The author also shows a regrettable penchant to complicate his exposition with word plays that leave the reader feeling that the pun is the lowest form of argument. But the book is nonetheless bold in its reach and conception, and the author not only provocative in his judgments but sensitive and sophisticated in his approach. He has portrayed with vividness and excitement an era in Japan's intellectual history which before seemed inordinately dull to this reader.

PETER DUUS
Claremont Graduate School

Vietnamese Anticolonialism, 1885-1925 *by David G. Marr. Berkeley, University of California Press, 1971.—xix, 322 pp. $12.50.*

Without claiming to know of all the books on Vietnam that have appeared in the West in recent years, I can say that more than five hundred new titles have come to my attention during the last decade. A few of these are general surveys of Vietnamese history; some describe French rule and its end after the Indochina war. The vast bulk of this literature, however, is journalistic accounts, attempts at political analysis of the regimes in South and North Vietnam, or mere polemical tracts dealing with American policy and events in Vietnam since 1954.

David Marr's book falls into none of the categories in which these writings might be grouped. As the work of an American scholar, it is a unique and, in the true sense of the word, pioneering achievement. To make this clear, it is important to underline what this book is not trying to be. It is not a comprehensive history of Vietnam between 1885 and 1925, and its emphasis is not on economic or social issues under colonialism. It centers on the intellectual, psychological, and cultural aspects of Vietnamese nationalism and is primarily concerned with those Vietnamese who resisted French rule. As such, however, it is unique for several reasons.

The first of these reasons is the sources Mr. Marr was able to use, sources unavailable to writers on Vietnam who do not know the language or who fail to realize the growing importance of Vietnamese language sources for the writing of Vietnamese history. (These sources, most of which were published in Hanoi and Saigon only after 1954, are listed in more than 700 footnotes and an extensive bibliography.) In addition, Mr. Marr has used a great deal of new French archival material as well as

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Chinese and Japanese sources to substantiate conclusions which, if necessarily tentative and partly no doubt controversial, are nevertheless supported by data no future historian of Vietnamese anticolonialism can afford to ignore.

Because he had access to these new sources, Mr. Marr was able to give us not only fairly complete biographies of the most outstanding and many minor leaders of Vietnamese anticolonialism, but also a great deal of information about the intellectual, cultural, and moral complexity of the anticolonial movement. From its beginning in 1885 as a traditionalist and proroyal struggle (the so called Can Vuong movement) led by uncompromising members of the scholar-gentry (mandarins) through the phases aimed at achieving its goals through modernization and the adoption of Western ideas, the anticolonial movement never achieved unity of purpose and action. It was always torn between archconservatives and antitraditionalists, with the latter again divided into clandestine activists and agitators for radical reforms of the colonial condition.

In describing, as no Westerner has before him, the lives and achievements of Phan Boi Chau and Phan Chu Trinh, Mr. Marr has analyzed the conflict between the activists and the reformists not only with impartiality, but also with deep sympathy, without in the least hiding the shortcomings of these two great representatives of anticolonialism or minimizing their tragic failure.

There is much that is new also about the Vietnamese exiles and students in Japan, adding to the picture of false Vietnamese expectations of help from Japan and the disastrous consequences for the anticolonial movement when Japan's true aspirations in Asia became evident. No less surprising is the wealth of new material in the chapters on the anticolonial attitudes and activities of the Dong Kinh Nghia Thuc (Free School of Tongking). This contains many translations from pamphlets, speeches, and bits of poems, all of them testifying to the bravery of the minority that chose resistance and thus inspired the next generation, one free of all attachment to the past and eventually also successful in overthrowing the colonial regime. A brief sketch of Ho Chi Minh's youth and early activities in Europe, the Soviet Union, and China up to 1925 precedes the conclusions Mr. Marr draws from his study, thus perhaps indicating that the author will continue his work and produce also the story of Vietnamese anticolonialism from 1925 to the movement's problematic triumph in 1954.

Although not a work for the general public, it is to be hoped that Mr. Marr's study will reach an audience considerably larger than the still small community of scholars and students engaged in related research.

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Anyone interested in the deeper causes of Vietnam's tragic fate over the last quarter of a century, be he teacher, journalist, politician, or merely concerned citizen, could not but profit greatly from a work whose new and relevant background information is not to be found in any other book on Vietnam.

In the spirit of true scholarship, the author says he intended to set others to challenging his data and conclusions, adding that he not only expects but even hopes that his work "will be rendered obsolete within the decade." I cannot share this broadminded optimism in regard to the progress we can expect in the field of Vietnamese historiography. I believe that ten years hence this exceptional book will be regarded not only as the pioneering work of a non-Vietnamese, but also as relevant to an understanding of Vietnamese nationalist attitudes as it is today.

JOSEPH BUTTINGER
New York City

Army Officers in Arab Politics and Society *by Eliezer Be'eri. New York, Frederick A. Praeger, 1970.—xii, 514 pp. $9.50.*

Military and Politics in Israel: Nation-Building and Role Expansion *by Amos Perlmutter. New York, Frederick A. Praeger, 1969. —xiv, 161 pp. $6.50.*

Dr. Be'eri's book, first published in 1966 in Tel Aviv in Hebrew under the title *Ha-ketzuna we-ha-shilton ba'olam ha'aravi* now translated by Dov Ben-Abba, appears with a few changes and corrections and many additions about the events of 1966 and 1967. It is a most substantial treatise about officers in the armed forces of Iraq, Egypt, and Syria since the mid-1930s. It details who they are and describes their social and class origins and connections, their educations, their family and marital connections, their political party membership, their philosophies, and their ambitions. Brief chapters also describe similar figures for Lebanon, Jordan, Yemen, and Sudan. The officer movement in Libya had not yet surfaced when the book was written. Other sections of the volume, dealing with the "patterns" of the military coups in the Arab states and with the "heritage of history" in the Middle East, try to establish political theoretical and scientific philosophies for these coups and a kind of historical tradition for military states.

Since most of these events, however, have occurred practically within the present generation, it seems that generalizations are open to question,

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especially with such varied societies and differing backgrounds as exist in the area. Over the last century and one-half—say six or seven generations—social, economic, educational, and cultural phenomena in Cairo or Alexandria can hardly be compared or equated with those in Baghdad or even in Damascus. There are nevertheless, very few errors of fact in this work. Some interpretations are open to question: though janissaries in the Ottoman empire seem to be similar in many ways with mamluks in Egypt, modern scholars such as Stanford Shaw and Halil Inalcik find more differences than otherwise; and to separate Druze from Muslim and thus possibly to infer that Druzes are non-Muslims is a frequent Israeli error.

The last two sections before the conclusion consider the motivations of the Arab officer-politicians, the actions of those who became rulers, and the elusive topic of Arab socialism. A broad reading of the mountains of data submitted by Dr. Be'eri reveals that Arab officers and civilians, alike, romanticize Arab unity and nationalism and give these ideals fervent lip service in public speeches but in making pragmatic policies follow a parochial course of action. In view of Dr. Be'eri's Israeli experiences and his socialistic beliefs, this volume is remarkably free from prejudice. He obviously feels that Arab officers in government and society are poorly organized and almost totally without constructive long-range plans for national development. He believes that most confuse hopes and dreams with reality, and he labels most as cynics.

Dr. Perlmutter, on the other hand, presents a straight historical account of the Jewish-Israeli military forces and army from the late nineteenth century to 1968. He relates the role of the military in building the modern nation of Israel. In essence, Dr. Perlmutter sets out as implied in his preface to show "the persistence of civilian over military authority, notwithstanding Zahal's [army] extraordinary role as an instrument of nation-building" (p. ix) and the "system of alliances between the military and society in Israel" (p. xi). But then the author proceeds to demonstrate step by step how the military leaders augmented their influence and power to the point where they were able finally to oust unsympathetic or uncooperative cabinet ministers.

There seems to be no question that the army did not participate in party politics, but Ben Gurion removed all officers with leftist views since they did not support his ideals of what the nation should be. Dr. Perlmutter states that the army in the period between 1948 and 1955 determined "to a large extent the course of Israel's foreign relations" (p. 81).

The well known Lavon affair (within Israel it is still highly classified

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material, undoubtedly because its revelations would embarrass Israeli leaders) was the successful ploy of the army against Defense Minister Lavon who was attempting "to control strategic policy making" (p. 81) and to wrest it from the army. It was resolved by bringing back the civilian Ben Gurion, who allowed General Dayan to control policy making. The latter's policies hinged on the ideas of Arab encirclement, retaliation, and preventive war, whereas Sharett and Lavon believed in conciliation and patient diplomacy to penetrate the Arab wall of hatred. Dayan and his fellow "heroic" officers looked upon the Defense Ministry as staffed by "clerks and merchants" (p. 56), which says a good deal about his attitude toward civilians. Likewise in 1967 when General Rabin could not get orders to strike from Defense Minister Eshkol, who did not make the proper "resolutions" and who "fell short of his job" (p. 120), pressure was put on to have General Dayan put in that position. Three days later he "ordered Zahal to strike" (p. 112).

In many ways the army has taken over and established a "garrisoned" state and submerged the older socialist views held by the early Zionists and the founders of Histadrut. The army role is that of "the maker of citizens, the inculcator of civic and nationalist culture" (p. 66). It takes charge of youth culture, civic values, ethnic integration, and social mobilization (p. 131), and has created the "heroic type" for officer and youth and idealized the "tough Jew, the image from which the nationalistic folklore of Israel has sprung" (p. 5).

Dr. Perlmutter sees no danger in these developments and lauds "the emergence of personal alliances between civilian and military elites in Israel" (p. 132), and the army's intricate and complex relationships with Israeli society. He seems to approve of the army's ousting of Lavon by condemning him because he "meddled in military policy" (p. 87). In the end, a careful reader will understand the powerful role the army plays in Israeli governmental and societal decision making and realize that Dr. Perlmutter approves. To equate Dayan's position and role as a civilian in government with Eisenhower's is a great mistake. General Patton would have been more suitable!

SYDNEY NETTLETON FISHER
The Ohio State University

Palestine and Israel: The Lethal Dilemma *by Hisham Sharabi. New York, Pegasus, 1969.—xi, 224 pp. $6.95.*

The Arab-Israeli Confrontation of June 1967: An Arab Perspective *edited by Ibrahim Abu-Lughod. Evanston, Northwestern University Press, 1970.*—xiv, 201 *pp. $6.75.*

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De Gaulle, Israel and the Jews *by Raymond Aron. New York, Frederick A. Praeger, Publishers, 1969.—160 pp. $5.95.*

In the preface to his volume, Ibrahim Abu-Lughod of Northwestern University asserts that in the "more than two years [which] have elapsed" since June 1967, "not a single scholarly, objective, and detached investigation of the factors . . . [leading] to that confrontation has appeared in the English language" (p. ix). To counterbalance the trend of the literature on the subject which has appeared to date and "to present an alternative perspective" (p. x), Abu-Lughod and Hisham Sharabi of Georgetown University have prepared two of the books under review. The work by Raymond Aron, a noted French sociologist and publicist, is so markedly different in both tone and content as to deserve separate treatment.

The Arab authors proceed from the assumption that the steady decline of Western prestige and the concurrent growth of Soviet influence in the Arab East are attributable to the misguided policies and pro-Zionist sentiments displayed by most American presidents in the post-World War II period. More precisely, they contend that, preoccupied with what was regarded as an imminent Soviet threat to the Middle East and unwilling to antagonize the powerful Jewish minority in the United States, official Washington has driven the Arabs into the hands of the USSR.

There is some truth in this but, as is often the case, reality is more complex than the stereotype. Put simply, the Arabs were not "innocent bystanders"; they, too, helped shape and participated in the events taking place in the Middle East. To argue that they were not full-fledged actors but passive onlookers is to grossly mislead the reader.

Another general theme revolves around the successes which the Israeli and Zionist propaganda organs have scored over their Arab counterparts in the competition for sympathy in the Western world. It is, of course, true that much of the news which reaches the average Western reader or viewer is often biased or simply false. As the authors contend, some of the responsibility for this state of affairs can no doubt be attributed to the pro-Israeli sentiments shared by what might be broadly described as America's intellectual elite. However, this cannot explain why the Arabs themselves, when given an opportunity to present their case, so often even fail to make an effort. One is reminded, in particular, of a recent attempt by CBS to obtain Cairo's viewpoint on a number of questions dividing the Arabs and Israelis. While the team dispatched to Egypt was given the bureaucratic run-about and was forced to return emptyhanded, the team which visited Israel came back with a report. The result was a "one-sided" exposition of the crisis.

On a more damaging level, the authors have fallen victim to the same kind of temptations for which they so roundly criticize others who have

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written on the topic, namely, the substitution of ethnocentric stereotypes for scholarship. This is particularly true of Sharabi's account of the situation preceding the outbreak of the June war. In a nutshell, his case can be summarized as follows: in May-June 1967, Egypt, because of its heavy involvement in Yemen, was not in a position to wage a war against Israel (p. 111). This being the case, Israel should have left Nasir alone, for he was simply responding to political pressure exerted on him by the other Arab leaders. (Why Israel should have been so obliging is not explained.) Moreover, Sharabi (p. 107) and Abdeen Jabara (Abu-Lughod, p. 169) appeal to the objective reader to accept at their face value the warlike statements of the Israeli officials and to dismiss as meaningless the threats to destroy Israel emanating from various Arab capitals. The reader may or may not be swayed by this argument, but it is naive to expect the Israeli government to maintain a cool and dispassionate attitude in the face of this type of a protracted and intense propaganda barrage. Finally, to argue that "the closing of the Gulf of Aqaba to Israeli shipping . . . was considered by Egypt as neither irrevocable nor as an act inevitably leading to war" (Sharabi, p. 111) is meaningless in the light of Tel Aviv's repeated warnings that such an action would be regarded as *casus belli*.

The book by Aron is a collection of articles written between 1960 and 1967. It contains his reflections on Israel and the Jewish question generally and constitutes a remarkable piece of soul-searching by a "'dejudaized' Jew examining his own conscience." His emphasis is on France and, more specifically, on the shift of French policy from full support of Israel in 1956 to veiled hostility in 1967. While disagreeing with the reasoning which underlay the change, Aron accepts it as a manifestation of de Gaulle's "new look" in the Arab East. Whereas, in 1955, France was preoccupied with its hopeless Algerian entanglement, by 1967 Paris had embarked upon its "bridge-building" policy in the Arab East. This, Aron admits, did not signify an abandonment of Israel but simply reflected the general's determination to reestablish a measure of influence in an area of traditional concern to France.

De Gaulle's famous press conference of November 27, 1967, however, with its allusion to the Jews as "an elite people, self-assured and domineering," fell into a different category. Aron condemns it as irresponsible and morally reprehensible because it encouraged anti-Semitism for what could only be described as political reasons. In this part of the book (pp. 19-49), he is at his most passionate and persuasive.

Looking at the Arab-Israeli confrontation from a historical perspective, Aron, despite the fact that his views were certain to earn him hostility in Zionist circles, is at his most perceptive. He notes, for example, that

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"in the eyes of non-Arabs, to recognize the existence of the State of Israel is to admit a fact; in Arab eyes, it is to accept an injustice and admit a defeat" (p. 103). How can these seemingly irreconcilable positions be brought into harmony? Not surprisingly, no one has come up with a magic formula. In the meantime, preoccupied with matters of self-preservation, conflicting interests, and prestige, the governments participating in the Middle Eastern drama jockey with each other in an attempt to secure advantages and establish positions of influence. In the process, they disregard the interests of the Palestinians—the real victims of their mutual intransigence. To Aron, the position of Israel, a relatively powerful but small island surrounded by a hostile Arab sea, is precarious for, to ensure long-term survival, it will have to achieve peace and, hopefully, reconciliation with its neighbors. Aron repeatedly questions whether this goal is brought nearer by open hostility and periodic wars. What the future holds no one can predict, but on the basis of the evidence contained in these three books it is difficult to be optimistic.

O. M. SMOLANSKY
Lehigh University

The Young Turks: The Committee of Union and Progress in Turkish Politics, 1908-1914 *by Feroz Ahmad. Oxford, Clarendon Press, 1969.—xiv, 205 pp. $7.00.*

Politics and Government in Turkey *by C. H. Dodd. Berkeley, University of California Press, 1969.—xiv, 335 pp. $7.95.*

Each of these books is concerned with the governing of Turkey. Each deals with a limited and politically agitated span of years following a revolutionary change in the government. In each change army officers took a leading role. There the basic similarities of the two studies cease, although smaller points can provide interesting parallels or contrasts. Ahmad, recently teaching at the University of Massachusetts in Boston, has written, as his subtitle proclaims, a new, concise history of the CUP during six years. Dodd, senior lecturer in government at the University of Manchester, conceals under his rather ambitious title a competent study of political and administrative organization and functioning in the Turkish Republic from 1961 to 1965.

The major points to emerge from Ahmad's account are that the CUP was no monolith and that the Istanbul government after 1908 (and, emphasizes Ahmad, after the 1909 counterrevolution was suppressed) was no one-party dictatorship. Neither conclusion is new, but both are better

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demonstrated here than they have been elsewhere. His book makes clear the continued importance of non-CUP and of anti-CUP forces, both organized and inchoate: senior military men, old-line statesmen, newly formed parties, occasional conspiracy. Only after considerable political vicissitudes did the Balkan War of 1912-13 give the CUP opportunity to concentrate power at the summit. Ahmad describes the constitutional changes in this period and refers rather briefly to the reforms undertaken, but politics is his central theme. Probably his second most valuable contribution is to document the CUP's shift from favoring centralization and repression of minority distinctiveness to a greater leniency in these matters, especially when the Balkan losses left the Arabs as the chief minority group.

The Young Turks, aside from some minor errors and internal contradictions, has three defects. First, the author never says *who* the CUP is, and the reader is left wondering until the last chapter, a summary, throws a little light on the question. Ahmad also speaks of the CUP's broad social base, but attempts no real examination of this. These are tricky questions, but they deserve an attempted answer. Second, the reader of this compact book often is left asking *why* something happened; more explanation is needed, even when the facts may be obscure. Why, for instance, was Mahmud Şevket assassinated? Finally, the author gives no explanation of why, though he admits he worked in Istanbul libraries, he did not use the Istanbul archives for his short six-year span. The basis of his account is English reports in the Public Record Office, which he uses well, supplemented by much Turkish newspaper and memoir material and the appropriate secondary literature. The total result is a good book which might have been still better. It is already selling in Turkish translation in Istanbul.

Although Dodd furnishes a short historical background, this is not central to his purpose and is sprinkled with little errors. He really begins with some highly intelligent speculation on the origins of the 1960 coup and what it signified. Then follows a chronological account of the coalition and caretaker governments from 1961 to 1965, when the Justice party won a majority and took over. The bulk of the book—about two-thirds—analyzes political organization and functioning in this period (with some glances back into earlier years): constitution, parties, pressure groups, elections, parliament, council of ministers, presidency, central administration, departments, provinces, and civil service. Only village government and municipalities are slighted.

These sections of the book on governmental organization are based in part on research Dodd did himself during three years in Turkey, coupled with data from many Turkish government publications and private

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studies. They are full of useful information, workmanlike, and usually dull, though enlivened by judgments that are sometimes quite felicitous ("The way to get the best out of Turkish colleagues is to let administrative affairs degenerate to the somewhat critical and then tackle them," p. 303) and sometimes unfortunate ("A deeply divided and agitated parliament has been an aid to political stability," p. 209 or "Turkish revolutionaries have always had very responsible attitudes towards the institutions of government," p. 313). Dodd might find cause in the events of 1970-71 to revise these views. This indicates the chief weakness of his book: much of it is now dated, though he has added a brief postscript on events to 1968. It remains a good study of 1961 to 1965.

RODERIC H. DAVISON
George Washington University

Uses and Abuses of Political Power: A Case Study of Continuity and Change in the Politics of Ghana *by Maxwell Owusu. Chicago, University of Chicago Press, 1970.—xxii, 364 pp. $11.50.*

Kwame Nkrumah: The Anatomy of an African Dictatorship *by T. Peter Omari. New York, Africana Publishing Corp., 1971.—xix, 229 pp. $8.50.*

After the plethora of books about the politics of Africa emanating from the pens of expatriate scholars, it is a welcome change to have accounts of their own countries by two Ghanaian scholars, Maxwell Owusu and Peter Omari (though it is not without significance that both are teaching or working abroad, having left the country for work or study in the period of Nkrumah's rule).

Omari's book is dedicated to J. B. Danquah, who died in Nkrumah's prisons, and it has a foreword by another noted opponent to that regime, Justice N. I. Ollenu. It is a slight and rather generalized account of the recent political history of Ghana, derived mainly from secondary sources, though bearing the mark of the author's discussions with high civil servants and others close to the centers of power.

Omari attempts to produce a balance sheet of the costs and gains of Nkrumah's rule in Ghana from 1951 to 1966. On the adverse side, he places much blame on the legal profession who "did not uphold the highest traditions of the judiciary, from the Amadu Baba case to the last judgement delivered before the coup." No doubt they fell short; but it is not easy to plot another course for them, especially when so much power

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was concentrated in government hands by economic and political measures.

Owusu's book displays quite the opposite characteristics. Whereas Omari is free of footnotes and of sociological theory, Owusu uses the bottom of the page as if it were a receptacle for evidence and for afterthoughts; meanwhile the main body of the text itself is often found sprinkled with references to Weber and Apter, together with a sometimes opaque use of a terminology that derives both from the Harvard department of social relations and from the University of Chicago. Owusu's central thesis is "simply that throughout the political history of the peoples of Ghana, from the heyday of traditional politics represented by the Chief and his elders, through colonialism represented by the governor and indirect rule, to the era of one-party state under Nkrumah and the CPP, *the primary basis of politics has continued to be instrumental*," that is, organized around material interest. In pursuing his argument, Owusu inevitably sets himself against the charismatic view of Ghanaian political activity (at least from 1951 to 1957), associated with analysis of Apter and others, which has stressed consummatory values, or "the concern with the ultimate." In a generous forward, Apter suggests that the difference of approach is partly related to the different points of analytical departure, his study having been concerned with the national scene, Owusu's with the local politics of Swedru; his with the contemporary events of 1952, Owusu's with the past happenings of 1964. He goes on to suggest that the truth may lie in between, that it was precisely Nkrumah's ability to combine the practice of instrumental manipulation with the theory of ultimate values that kept him in power.

Owusu certainly makes his point about the manipulative aspects of Ghanaian politics being relevant at all levels and provides some interesting examples of situations whereby "custom was used in the service of nontraditional economic and power interests." But the value of his study lies not in the polemic with Apter, which either deals in crude alternatives or else depends upon the comparison of immeasurables. It lies in the exposition of politics at the local level, the way the system "really worked," the relationship of chief and trader, of foundation member and subsequent recruit, of military company and political party. On this level my own observations in other parts of the country would confirm much of what Owusu maintains, though even there "ultimate values" were of greater importance than he is inclined to admit. So too were "primordial ties," for if "custom was used in the service of nontraditional economic and power interests," it was also the case that local kinship and clique affiliations (e.g., the concept of the founder member itself) often overcame the pursuit of more immediate political or economic gain.

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These aspects of the situation do emerge in Owusu's analysis of Swedru politics, which makes his study so much more valuable than if it was simply a *roman à thèse*. Despite the hurried quality of some of the writing (most agonizingly revealed on p. 158 where he refers to the close relationship between chiefship, land, and religion which "has been amply demonstrated in this and other papers"), despite the occasionally repetitive and often dispersed treatment of specific topics, and despite the moments of inflationary perspective (where is the consideration of "the consequences of the research for policy formulation" announced in the introductory pages?), this is the best available account of grass-roots politics to have emerged from Africa, and it is particularly valuable in showing the manipulation of power (in some ways much less centralized than is often supposed) in a one-party system. As such it displays more of "the anatomy of an African dictatorship" than Omari's more generalized account.

JACK GOODY
St. John's College, Cambridge

Four African Political Systems *by Christian P. Potholm. Englewood Cliffs, N.J., Prentice Hall, 1970.—ix, 308 pp.*

Biafra: The Making of a Nation *by Arthur A. Nwankwo and Samuel U. Ifejika. New York, Frederick A. Praeger, 1970.—xii, 361 pp. $8.95.*

The Cameroon Federation: Political Integration in a Fragmentary Society *by Willard R. Johnson. Princeton, Princeton University Press, 1970.—ix, 426 pp. $12.50.*

Rwanda and Burundi *by Rene Lemarchand. New York, Frederick A. Praeger, 1970.—xi, 552 pp. $12.50.*

While these four studies reflect different approaches to the study of African political systems, they all specifically or implicitly are concerned with successful evolution from colony to independent state to viable nation-state. Published in 1970, all four reflect the events and theoretical innovations of the decade of the sixties. We are dealing here with organic and not mechanistic analytical approaches to political systems—systems that suffer from the afflictions of political instability and low levels of national integration—and the authors' attempts to prescribe preventive or curative measures that will preclude political decay.

Johnson's book examines the dynamics of the integration process as a

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requisite for stability in Cameroon. Potholm compares the four states of Tanzania, South Africa, Ivory Coast, and Somalia within the framework of a political development-political decay continuum, while the Nwankwo and Ifejika volume, written by two Biafran publicists during the heat of battle, explains the reasons for decay. It is Lemarchand's comparative study of Rwanda and Burundi, however, that attains the apogee of organic theory. By suggesting that the traditional political culture and not imposed Western institutions will have the greatest impact on the nature and form of African political systems, he implies that the ontogeny not only recapitulates the phylogeny but possibly determines it.

Potholm's *Four African Political Systems* provides a more recent attempt to define a conceptual framework for comparative politics and the analysis of African political systems. The model suggested, very much in the tradition of systems capability analysis, identifies four areas of investigation: participation in the decisionmaking process, capability analysis, selection of goals and their relationship to the environment, and the stages of political development, e.g., traditional, transitional, or modern. One of Potholm's few innovations—that a political system may overlap territorial boundaries to include psychological boundaries—raises serious questions in the mind of the reviewer (Somalia and the Somali peoples of northeastern Kenya and Ethiopia is cited as an example). The notion of a political system assumes the existence of a geographically circumscribed entity composed of a variety of interdependent structures that perform certain prescribed functions. Such a concept of an interdependent whole rules out the inclusion of an external group, despite ethnic affinities, that is subject to a totally different system of interacting structures and functions. Moreover, his criticism of the structural-functional approach on the grounds of its tendency to equate the survivability of a political system with success, reflects a confusion of normative considerations with empiriral facts. This assessment that what works is successful need not imply that it is also good.

The author, who considers Tanzania the most encouraging of the four states, points out that there is no linear relationship between economic and political development. Consistent with the suggestion of Samuel Huntington in his seminal article "Political Development and Political Decay," Potholm reiterates that scholars might find it more beneficial to study models of political decay, stagnation, and atrophy rather than those of development.

In a departure from the Potholm and Lemarchand books, the authors of *Biafra: The Making of a Nation* attribute the devolution of the Nigerian political system not to indigenous factors, but to the impact of colonial rule. Criticizing the British for a "positive frenzy for the federal con-

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cept," they suggest that the Nigerian federation, like those that emerged in other British colonies, was created for England's economic and imperial gain. Ifejika and Nwankwo argue that Britain then encouraged regional autonomy and parochialism through territorial and administrative separation, a policy of indirect rule, unequal missionary evangelization, and unequal educational opportunity.

However, in attempting to justify Biafran secession, they occasionally lapse into irrationality. For instance, taking a geographical determinist position, they state that the characteristics of the "highly individualistic and productive people of the south" and the "conservative, less enterprising" northerners resulted from the existence of forests and swamps in the one area and open savannahs and semi-desert in the other. Elsewhere they suggest that unity should not be imposed but should evolve from peace. Yet historically, as pointed out in William Riker's *Federalism: Origins, Operation, Significance*, it is not peace but the external threat to peace that has been the most important variable contributing to unity and federation and their permanence.

Ironically, the authors view a confederation to be the viable alternative to federation. It would appear that a confederal solution would duplicate the colonial political pattern that exacerbated rather than attenuated ethnicity and regionalism considered so odious by the writers.

Contrary to initial fears, the post-civil war period seems to indicate that the Nigerian political system has not decayed, but is entering a period of rebirth bolstered by a more efficient economy, stronger military apparatus and a greater unity of purpose.

Johnson's excellent study, *The Cameroon Federation*, marks a welcome departure in the analysis of African political systems. Unlike the first wave of primarily empirical political histories, this book assumes its reader is familiar with the basic data and extrapolates from it to give a dynamic analysis of the process of political integration in the Federation of Cameroon. Apparently concerned about correcting a situation where "theories rise and fall before they are subjected to adequate empirical tests, only to be replaced by new ones which suffer from the same deficiencies and are subject to the same fate," Professor Johnson rigorously scrutinizes the theories of political integration (Ake, Haus, Etzioni) and modernization (Apter, Weiner) and submits them to the acid test of survival in face of the empirical data on Cameroonian unification. In the process, he occasionally uses the elliptical prose that has been a concommitant of theorizing in the profession: "The invariant property of the system, which Nagel labels as G, is a function of the interactions of the second type, described as the state coordinates, which are those variable actions and properties that determine the state of the system" (p. 26).

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Johnson rejects Almond's notion that the level of integration is in itself a characteristic of culture. Rather, he concludes that a country may be without a "superlative political culture and embody a variety of political cultures in juxtaposition, conflict and competition." Therefore, cultural integration would involve creating a "pervasive" culture which overarches the others and ties them together. Thus, one of Johnson's basic points is that integration in Cameroon was based not on a common identity but a pluralistic identity and occurred because of a central executive with the juridical strength to exercise forceful control. It is surprising, however, that Professor Johnson continues to use such terms as "tribes," "pagan," and "vernacular" when referring to African ethnic groups, traditional religions, and African languages.

More restrictive in its scope than the Potholm study but more complete in its detail and the originality of the analysis, Lemarchand's *Rwanda and Burundi* purports to explain the paradox of two kinds of revolutions within the context of two similar political units and what this revolutionary experience can tell us about political change. Once again empirical facts challenge current theories. Both states refute the assumption that national unity results from many centuries of "shared historical experiences." Moreover, Lemarchand concludes that the "crucial variable" that explains the different political development in the two countries is not so much the "degree of decentralization or centralization of the traditional authority system as the degree of separateness between different status groups." Unlike Burundi which had a weak monarchy and greater social mobility, "monarchic rule and the hegemoniac caste system" were so intimately intertwined in Rwanda that any attempt to democratize social stratification would inevitably bring about the demise of the traditional monarchy.

In an excellent concluding chapter on "Revolutionary Change and Nation Building," Lemarchand sheds light on the role of the rural dweller in political change. Discounting both the concept of the totally passive or pivotal peasant, Lemarchand supports the notion that the peasant is mobilized through "involuntary victimization."

The states analyzed in these four books and most other African states face a crucial dilemma. The development of a stable polity, itself a prerequisite of political modernization, assumes a slowing down of mass mobilization to permit a sufficient concentration of power at the top. But, as Lemarchand observes, the problem is how to be "authoritarian enough to ensure rapid modernization and political stability and yet not too authoritarian so as to conjure up memories of the ancient regime" (p. 492).

HERSCHELLE S. CHALLENOR
Brooklyn College

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The Angolan Revolution, Vol. I: The Anatomy of an Explosion, 1950-1962 *by John Marcum. Cambridge, Mass., The M.I.T. Press, 1969.—xiv, 380 pp. $12.50.*

John Marcum has written a remarkable and exhaustive study of the Angolan independence struggle up to 1962. A second volume will presumably take the history of this struggle to the present. The greatest merit of the book is the fact that the author clearly spared no effort to set down the history of this little-known and important African revolution in every detail available to him. He has avoided coming to the material with a preconceived hypothesis or even a methodological predilection. The result does not make easy reading. We are introduced to scores of small groups and parties all with their abbreviated names, but this is a necessary price if the writer is to present a comprehensive picture without immediately imposing his own evaluation on the material by excluding what he considers unimportant. Social scientists who have the good fortune to deal with material which has not been seriously analyzed before would do well to recognize their special responsibility to set down the facts first.

The Angolan Revolution is important both because it constitutes one of the few examples of the sustained use of violence as part of an African independence struggle and because its failure, up to the present, puts into question the generally accepted analysis of the post-World War II success of African nationalism—mass mobilization by modern elites overcoming weakened European colonial powers which no longer have the ability or will to resist. Why was the weakest of these powers able to do so? Was the nationalist thrust different? Was the colonizer different?

Marcum's book first traces early manifestations of protest against Portuguese rule. Africanists will not find the Angolan experience markedly different from patterns set elsewhere in Africa for this phase. Protest occurred in the context of three separate ethnic clusters or combinations —the Luanda/Mbundu, the Bakongo, and the Ovimbundu/Chokwe. Each developed a set of different organizations and also expressed protest in a variety of ways, through elite groups, peasant upheavals, and messianic movements. There were also ideological splits among the elite, with some intellectuals having had early contact with the French Communist party and others reflecting Catholic and Protestant origins.

The main distinction of the Angolan situation is the infinitely sharper and more oppressive and more determined reaction of the colonizer. One result of this difference has been that the search for unity has been very difficult. Although Marcum seems to suggest that a large part of the responsibility for this failure to achieve unity among nationalist forces must be borne by Holden Roberto, the leader of the UPA, it is possible to

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deal with the question at a more abstract level. Initially, the need for secrecy was so great that not only could movements not really communicate with one another, but they often did not even know of each other's existence. Later, the whole concept of unity became more complex as a result of the harsh conditions under which Angolan nationalists were operating.

Marcum shows how the most important split has been between

> two major exile movements that competed for revolutionary leadership. [The first involved] exiled leaders of two loosely allied parties (UPA and PDA) within the northern Bakongo stream of nationalism [who] formed a Government in Exile (GRAE). The GRAE was then strengthened as a number of nationalists from the central Ovimbundu and eastern Chokwe stream . . . joined one of its constituent parties. . . . [The second was] the MPLA, a product of the urban, multiracial Luanda-Mbundu stream of Angolan nationalism, [which] sought alternately to displace the GRAE's military force inside the country and to persuade the GRAE two-party coalition (FNLA) to join it in a three-party common-action front (p. 318).

The division between the two tendencies also reflected differences in the backgrounds and orientations of their leaders. The MPLA had a "well-educated largely Marxist and mulatto leadership" (p. 318); the GRAE, a more empirical and less well-educated leadership. This in turn resulted in the identification of Angolan opponents with cold-war camps, the MPLA receiving its main foreign support from radical Third World movements and states and the GRAE from moderate African states such as Tunisia. The GRAE also tried to find substantial support in the U.S., but in the crunch found that the U.S. was willing to pay lip service to its goals of independence but very little more. The NATO alliance and the U.S. bases on the Azores prevailed over principles of self-determination.

While the major portion of the book deals with nationalist organizations, Marcum emphasizes the importance of grass-roots protest. The most spectacular example was the peasant uprising in northern Angola which began in 1961. Although it was only the ultimate outburst of what the author describes as a "sequential explosion," it is this event which has given the Angolan struggle its special character and legitimizes the use of the word *revolution* to describe it. Marcum shows that the UPA later led this uprising, but it seems unclear exactly how it started and to what extent it was really disciplined by any organization. Unfortunately, the author sheds relatively little light on the internal dynamic which kept the rebellion going. Here he is too prudent. Obviously, it is extremely difficult to obtain hard data on a peasant rebellion, but that only increases the importance of presenting whatever information is available. And Marcum who had the courage to enter rebel-held trrritory, gives us

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very little of his own observations.

This reluctance to bring himself into his book is, to this reviewer's mind, a fault which appears elsewhere as well. It is, for instance, no secret that the author was on intimate terms with the UPA leadership, especially Holden Roberto. Did this not produce special advantages for his research on this movement, and special obstacles when he investigated other parties?

These minor criticisms should not, however, detract from the major contribution which John Marcum has made both to the sociology of peasant uprisings and to the history of Africa's struggle for independence. The great effort and honesty which has been put into this book is everywhere apparent.

HERBERT WEISS
Brooklyn College, City University of New York

Pre-Colonial African Trade: Essays on Trade in Central and Eastern Africa before 1900 *edited by Richard Gray and David Birmingham. London, Oxford University Press, 1970.—308 pp. $7.00.*

This book is a collection of thirteen essays, all written by different authors, nine of which derive from research presented to seminars at the School of Oriental and African Studies, University of London, and at the University of California at Los Angeles. Some of these contributions have already appeared in print, and in assembling them within the covers of one book, the editors might convey the impression that there are common denominators in these diverse and unequal treatments of an important and emerging topic in the developing history of Africa. In point of fact, the common element in all of these essays is that they concentrate on the period before 1900; the book includes essays on the early trade of south central Africa, Nyamwezi trade, the trade of southern Ethiopia, Loango in the seventeenth and eighteenth centuries, and the early trade of Angola, among others. All attempt, in what are essentially monographic studies of differing topics and of separate ethnic groups, to describe the pattern of indigenous trading that evolved before the advent of Europeans. In a collection of this type it should occasion little surprise if the quality of the essays is uneven; some are brilliant, throwing new light on aspects of precolonial trade generally, while others are too localized to be of much value in assessing the pattern of trade in a wider area. Some describe subsistence-oriented trade, a system in which trade remained closely tied to subsistence agricultural production and was subservient to the local kingship system. Others describe the phase in which trade

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freed itself from subsistence and kingship shackles; market-oriented trade developed and created a complex system of economic innovations brought about by new commercial opportunities. Still others emphasize the importance of internal trading routes as predecessors and complements to external trade.

It is on materials such as these that the editors base some of the ambitious but stimulating generalizations on the character and pattern of precolonial African trade discussed in their introductory chapter. One wonders after reading the material available in this book and comparing it with the situation in contemporary West Africa (where subsistence economies and long-distance trading had existed hand in hand for centuries) whether sufficient research has been done to warrant the generalizations which the editors cautiously attempt in the introductory chapter.

If economic history is to make a significant contribution to our understanding of the African past, historians must concern themselves, as the editors have, with the study of the continuities between precolonial economies and the present and investigate further what the editors have termed "an intermediate category" located "midway between subsistence and fully-fledged market economy." Until recently historians have shied away from the economic problems involved in their study of indigenous society and have tended to concentrate heavily on the political and diplomatic aspects of African history. Because of this, recent research has thrown little light on the dynamics of some important aspects of African historical development. This book is therefore a step in the right direction and, we hope, a precursor of other publications in African economic history.

K. Onwuka Dike
Harvard University

Trade and Politics on the Gold Coast, 1600-1720: A Study of the African Reaction to European Trade *by Kwame Yeboa Daaku. Oxford, Clarendon Press, 1970.—xviii, 219 pp. $8.00.*

An extremely useful reworking of a doctoral thesis written at the University of London, this book is almost perfectly titled. I say "almost" because it does not stop with the admirable effort to examine African reactions, the importance of which historians now, fortunately, accept. This book goes a necessary step further. It shows that not only were the African factors crucial, they were often as active as the European ones. The

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author should give his study credit for that perspective; the eighteenth-century Gold Coast's story is of European reaction to African politics almost as much as African reaction to European trade.

Not that the two are separable. And, in placing his main emphasis on trade, Daaku has made a particularly important contribution, for, compared to political analysis of the African past, economic history has been relatively neglected. In following his economic themes, the first service the author performs is historiographical. Though not absolutely up to date, he does examine critically and constructively writings that have gone before, from those of the earliest European observers in the seventeenth century through those of historians in the mid 1960s. Into this carefully evaluated background he feeds material gained from the traditional histories of the peoples of the former Gold Coast. The synthesis he provides, not only of coastal history, but of what is now known of the rise and fate, in the seventeenth and early eighteenth centuries, of the inland confederations of states, results from thorough and thoughtful analysis and is valuable in itself.

The author's most original contribution comes from his having mastered Dutch and worked carefully through Dutch archival material, especially the contemporary records of the Dutch West India Company. His use of the Treasury papers in Britain, a particularly difficult set of public records to work with, has also thrown new light on the activities of the seventeenth-century European traders on the Gold Coast, and on their interactions with the Africans. Again, relating the African side of the tale, drawn with careful correlation from oral evidence, gives a three-dimensional picture not previously available.

Much of the work is quite detailed. The reader is offered the particulars of different kinds of trade, in gold and ivory and in slaves; the details of rivalries between traders of different European nations, especially the English and Dutch after the Portuguese influence diminished; the working out of similar competitions between African states, coastal and interior—all rivalries couched in economic as well as political explanations. He sees African manipulations of Europeans and the reverse; understandings and alliances reached; and the depths of misunderstanding over, especially, differing ideas of land tenure so common wherever cultures meet and knowledge is partial.

The author considers, too, the social and cultural changes the European presence triggered (the importance of guns, though not a new discovery, is set in slightly broader perspective than before too). Particularly interesting is the role played by a few African merchants of a new type, who became political as well as economic middlemen, responsive to

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African initiatives from the interior as well as European ones from the coast.

Excellent maps, redrawings of contemporary ones as well as the originals, appendixes containing some key agreements signed on the coast, and a good index add to maximizing the contribution of this valuable study.

JEAN HERSKOVITS
College at Purchase, State University of New York

Politics and Change in Developing Countries: Studies in the Theory and Practice of Development *edited by Colin Leys. New York, Cambridge University Press, 1970.—xi, 289 pp. $7.50.*

Colin Leys is nothing if not audacious. He begins his introduction with a quotation by Lamb denouncing biblia a-biblia, and gives us as the rationale of his book: "It is by British writers; and it has to do with the so-called 'revolution in political science' as it bears on the study of development" (p. 1).

The first is a somewhat curious justification. We are used by now to having calls for a distinctively Black viewpoint or a Latin American viewpoint or even a radical viewpoint. But are we now to be thought to be missing a British viewpoint, too? Can it be that after three centuries of dominating the seas and playing no insignificant role in letters and science, the British feel neglected? Is this why Winston Churchill declined to preside over the demise of the empire?

The second task seems more legitimate, especially since it requires some courage to defend political behaviorism at the very moment when it is under most severe attack. One must say Colin Leys does it with balance and intelligence. And I suppose that if the object of the exercise is seen as being "centrally concerned with theory and with understanding politics in terms which disclose their significance at the widest level of generality appropriate to the subject matter" (p. 11), then I cannot fault the exercise.

But the papers are not in fact intended to be expositions of this ideology nor do they particularly demonstrate its efficacy. They merely are a series of illustrative exercises in it. Another set of papers might have done as well. There are, to be sure, a number of good items in the book. I particularly liked Joan Vincent's survey of the reassessments anthropology has had to make of its own activities in the light of political develop-

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ments, as well as the study of these political developments, since the Second World War. I also enjoyed Martin Stainland's debunking, on the basis of a careful dissection of Ivory Coast politics, of the vacuity of much writing about single-party regimes, although sometimes I felt he was close to overkill. J. P. Nettl and Alec Nove give us shorter versions of previous major analyses. W. H. Morris-Jones gives us a brief preview of what will doubtless be a very useful analysis of recruitment patterns in the Indian National Congress.

There is, as one can readily see, a bit of something for many palates and a tolerance of idiosyncratic ways —a British contribution, perhaps.

IMMANUEL WALLERSTEIN
McGill University

The New Left: Six Critical Essays on Che Guevara, Jean-Paul Sartre, Herbert Marcuse, Frantz Fanon, Black Power, and R. D. Laing *edited by Maurice Cranston. New York, The Library Press, 1971. —208 pp. $6.95.*

Few would quarrel with Maurice Cranston's contention that there are differences between the New Left and the older tradition out of which it has emerged. How the identity of a movement still so amorphous as this is defined, however, is bound to depend upon which authors and doctrines are admitted to the canon of orthodoxy.

The "luminaries" treated in these essays are said to share a number of characteristic attitudes. Their accent falls upon the philosophical and sociological aspects of Marx rather than his economic determinism. The concept of alienation is generalized to refer to the plight of the personality in "the Hell of the Affluent Society" (Marcuse) and not only to the separation of the worker from the product of his labor. A prophetic, often utopian style of thought replaces the effort to develop a "scientific" socialism; positivistic preoccupation with historical facts is condemned as uncritical and a reflection of conservative bias, especially when the facts refer to disillusioning experience with revolutionary socialism. Violence is glorified, in a way more reminiscent of Georges Sorel than of Marx. The working class is held to be corrupt and bourgeois, and therefore unfit for revolution. Instead, a new proletariat is identified among Frantz Fanon's "wretched of the earth"—the "third world" peasants from whom Marx expected no help, rebellious intellectuals and students who he thought could be useful only if they attached themselves to the working

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class, and even demimondaine types scorned by traditional Marxism as among the human refuse of the lumpenproletariat.

As yet, the New Left lacks a new Marx. Two of the prime candidates, Sartre and Marcuse, are examined in this book and pronounced unfit for the office. François Bondy points out that while Sartre has had a considerable influence, notably upon Fanon and R. D. Laing, he has never developed a fullfledged political theory. Before 1956, his politics consisted of enthusiastic endorsements of the party line mixed with comradely criticism; since then he has tried, without conspicuous success, to thread his way through the shoals of polycentric communism. Throughout, Bondy shows, Sartre has been far more concerned with defining "the correct attitude of the intellectual" than with elaborating a consistent theoretical perspective. As a result, Sartre is now identified with the rather tepid view that revolution would be a good thing, in the East as well as the West, so long as it does not entail the overthrow of Western capitalism or serve as a sanction for totalitarianism.

As much by default as on account of his work and influence, Marcuse emerges as the more likely bearer of Marx's mantle. Cranston therefore expounds his views in detail and argues that they fall far short of the master's standards. In particular, he observes that Marcuse betrays a penchant for Bakuninist anarchism and a snobbish dislike of the masses, that he bases his optimistic prophesies of "nonrepressive civilization" purely on aprioristic assumptions, that he resorts to the magic wand of "transformation" in lieu of explanation. Above all, Cranston complains that although Marcuse claims to be a humanitarian socialist, "he advocates intolerance and confers a blessing on violence."

Marcuse richly deserves excoriation, especially for his complicity in the New Left's caricature of liberal democracy as repressive authoritarianism. At this point in history, moreover, his expectation that tolerance and democracy will somehow result from a revolution carried out in defiance of these norms is either inexcusably naive or cynically deceptive. On these points, Cranston's rebuke is effective and appropriate. In other respects, his critique falls short. He neglects to note, for example, that Marcuse's "transformation" of Freud's *ananke* or necessity into "material scarcity" utterly distorts Freud's view of the grounds for restraining human instinct. Nor is it altogether fair to make no distinction between Marcuse's justification of revolutionary violence as historically necessary —in which he merely reiterates Marx's view—and Fanon's exhortation to violence as a purifying therapy.

The other figures treated in this book are similarly portrayed by their critics as victims of their own self-inflicted contradictions. Aristide Zolberg skillfully depicts Fanon as a tragic prophet, shaped by Western ideals

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but compelled by a messianic fervor to denounce the West as a racist and exploitative world-oppressor, his theory of violent revolution irrelevant to most actual experience of decolonization, his followers doomed to be disappointed in their expectation of secular salvation. Kenneth Minogue contrasts Che Guevara's elevation to political sainthood with his altogether unmiraculous achievements. Having produced the latest version of the Leninist sublimation of theory into practice to explain the success of the Cuban revolution, he came to grief because he tried to apply his own conclusions in the rather different conditions of Bolivia.

George Feaver hints at, but does not fully explain, the dilemma faced by American black militants, who must choose to define slogans like "black power" either in the conventional and pragmatic terms of American pluralism or as a hopelessly inappropriate but ideologically more satisfying expression of the universal revolt of the colonial masses. Robert Martin points out that for all R. D. Laing's success in communicating with the mentally ill, or perhaps because of it, he is incapable of presenting a coherent, logical analysis of society but instead must substitute "ecstasy for argument" and indulge in "random accusation and sloganised virulence," with the result that he himself adopts a viewpoint Martin neatly diagnoses as "macro-schizophrenia": the self hating the society that is most truly its own.

If the composite portrait that emerges from these six well crafted sketches fits the description offered in advance, it is at least partly because of the principle of selection. If Ernest Mandel and C. B. Macpherson had been considered, economic determinism would still be strongly in evidence. If Georg Lukacs, Adam Schaff, and Leszek Kolakowski had been treated, the links between old and New Left would probably also appear more strongly. If Paul Goodman, Norman Brown, Christian Bay, Theodore Roszak, Ivan Illich, and Charles Reich were included, the communitarian-anarchist facet of the New Left might emerge as the central tendency and it would be harder to claim that the glorification of violence is a common theme.

The principle of selection as well as the critical tone of the analyses clearly reflect the liberal bias of the commentators. And why not? Radicals always monopolize the rooftops; liberals should occasionally open the windows of their studies and shout back. It will take more than a book of essays to generate a new liberalism, but, as radicals might say, the criticism of the existing order is the beginning of its replacement.

SANFORD A. LAKOFF
University of Toronto

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Obligation: A Social Theory *by Ralph Ross. Ann Arbor, University of Michigan Press, 1970.—200 pp. $7.50.*

Professor Ralph Ross, of Scripps College and the Claremont Graduate School, is a philosopher of the old school. Co-author with Ernest van den Haag of the valuable introduction to the social sciences, *The Fabric of Society* (New York, 1957), he rejects a narrow view of the philosopher's function. He thinks that philosophers should be informed about other fields, like sociology or political science, and should have something of substantive interest to say to scholars in these fields and to the intellectually responsible citizen. His practice, as this new book attests, lives up to his general views.

Despite its subtitle, *Obligation* is not "a social theory." Professor Ross's concern is rather to elaborate a perspective toward society appropriate to our behaving as responsible moral agents, and he proposes not so much a general scheme of explanation of social events as a set of guidelines for our thinking and acting with regard to certain selected issues. Thus, he deals with the difference between "conventional morality" and "critical morality," the nature of political consensus, the social functions of the intellectual, the relation of politics to the wider social framework, and the moral dimensions of politics.

The heart of his view is that there is a phenomenon—obligation—which runs through our social relations and is not to be confused with relations based on force. To perform an obligation is to perform a free act, to do something by unconstrained choice; specifically, it is to choose to do what one's membership and status in a group requires. This, for Professor Ross, is the nub of a free man's relation to his society and the context in which his moral decisions are made. An adequate moral philosophy is a critique of one's obligations as one finds them. Its task is to ask how these obligations advance crucial values and how they can be reconstructed to serve these values still better.

Because Professor Ross emphasizes the element of free choice in obligation, he is particularly concerned to refute what he calls "deterministic" theories of human behavior. He scores heavily, I believe, in his criticism of simplistic views of human behavior such as have been put forward (at least sometimes) by scholars like B. F. Skinner; and he also makes the useful point that much so called determinism is the product of insulated intellectual specialization, which leads people to interpret human behavior within the categories of a single discipline and to ignore those features of behavior which fall outside these categories. Professor Ross's "principle of limited categories," which he uses as a general therapeutic device to counter such tendencies, as he recognizes, is much like

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Whitehead's "fallacy of misplaced concreteness."

Despite the cogency of much that he says, I have a number of questions about both the point and the effectiveness of Professor Ross's extended discussion of determinism. First, does the fact that a choice has causes prove that it is unfree? There is a difference between coercion and causation. Second, what exactly does *determinism* mean when it is used as an unqualified label? Different schemes of causal explanation—e.g., those of macrophysics and those of subatomic physics—provide different models of "deterministic explanation." The simple mechanistic model of determinism, associated with La Place, has long since been reduced to a minor role in the sciences, but Professor Ross, if I follow him, seems to have mainly this model in mind. Third, is determinism to be interpreted as though it were a descriptive theory? It can also be interpreted merely as a regulative principle of inquiry, instructing us to look for causes and not to take the failure to find them as a reason for cutting off our investigations. Professor Ross, in my view, does not give adequate attention to possible objections of this sort to his views.

But a full discussion of this matter would take us far afield, and my disagreements with Professor Ross do not affect my admiration for the judiciousness and balance of his judgments, or for the sensitivity of imagination with which he explores the moral issues involved in the performance of a free citizen's role.

CHARLES FRANKEL
Columbia University

Knowledge and Human Interests *by Jürgen Habermas, translated by Jeremy J. Shapiro. Boston, Beacon Press, 1971.—vii, 356 pp. $7.50.*

Professor Habermas is the foremost representative of what has come to be known as the second generation of the Frankfort school of neo-Marxist philosophy and sociology. For a while, the German New Left looked at him as a guide and beacon. But after he wrote of the dangers of Left Fascism in connection with some of the more outrageous actions of the student radicals, he lost much of his previous influence with them. This is indeed a sad fate for a philosopher who is programmatically committed to the joining of action and reflection, of *Praxis* and critical theory.

If I understand him aright, and this is by no means made easy by the involuted complexity of his exposition, he means to provide with this book the grounds for a thorough attack on the philosophical basis of

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positivism. Positivist thought, he contends, severs knowledge from interest, and erects a barrier between descriptive and prescriptive statements. In consequence, claims the author, "the conception of theory as a process of cultivation of the person has become apocryphal. Social theory is possible only as the self-reflection of the knowing subject." In self-reflection, knowledge for the sake of knowledge is wedded to the interest in achieving autonomy and responsibility; it is a movement of self-emancipation. Hence positivism, which excludes such reflection as illegitimate and defines the meaning of knowledge by what the sciences do, stands in the way of the emergence of a truly humane social theory.

The first part of the book deals with Kant's critical theory of knowledge, with Hegel's critique of Kant, and with Marx's metacritique of Hegel. Marx is said to have lacked a full philosophical self-understanding since, though his inquiries showed otherwise, he tended to conceive of his critical approach to society as a subset of the natural sciences: "By equating critique with natural science, he disavowed it." Not even Marx managed to escape the lures of positivistic scientism.

The second part is devoted to a long and highly technical discussion of Peirce's logic of inquiry and Dilthey's historicism. It would seem that neither of these philosophers managed to free himself from the positivist incubus.

The last part, which is mainly devoted to a discussion of Freud's mode of analysis, is the highpoint of the book. Faced with a thinker who formulated his thoughts with exemplary clarity, Habermas here abandons his murkiness of exposition and writes with cogency. Though Freud's mental equipment was rooted in an obsolescent physiological scientism, Habermas contends, Freud was led through his analytic practice to conceive of a mode of interpretation that found its justification in a process of enlightenment of the patient. Psychoanalysis has therapeutic results only if the patient overcomes blocks to consciousness; the patient is aided through self-reflection to appropriate lost portions of his life history. The psychoanalyst, by making it possible for the patient to replace id drives by ego awareness, makes himself the instrument of the patient's knowledge. In a similar way, I presume, Habermas conceives of the social theorist of the future as an agent of human emancipation who will stimulate the awareness of the subjects he studies. His critical thrust will aim to enlighten humanity through self-conscious reflection about the burdens and distortions of capitalist culture.

Professor Habermas seems unaware of the fact that a contribution to the self-emancipation of mankind has loomed very large on the agenda

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of many of those "positivists" he so despises. But that, plainly, is another story.

LEWIS A. COSER
State University of New York at Stony Brook

Political Thinking and Consciousness: The Private Life of the Political Mind *by Robert E. Lane. Chicago, Markham Publishing Co., 1969.—x, 348 pp. $6.95.*

An End to Political Science: The Caucus Papers *edited by Marvin Surkin and Alan Wolfe. New York, Basic Books, Inc., 1970.—viii, 324 pp. $7.95.*

Lane's achievement here is to have shown that politics is mediated for the individual by a unique, quarter-conscious but psychologically coherent, *outlook*, deeply rooted in personal needs and anxieties; and to have assembled a small but powerful kit of tools for its analysis. His raw material is 24 political autobiographies of undergraduates at Adams College in the late 1950s. They contrast nicely with his earlier interviews with dull, resigned working men in *Political Ideology*, and an avuncular concern for youthful promise warms the book, though we are far from any Eriksonian ingratiation. For good measure, an original and sustained essay threads the book on psychological concomitants of radicalism-conservatism.

On its own terms, *Political Thinking and Consciousness* is entirely successful—"reducing" the main features of these outlooks plausibly and parsimoniously to three or four major "needs" (approval, autonomy, aggression). A "new approach to political thinking"?—certainly; and one with wide implications. Yet, as one reflects on the specialists who might, ideally, be stimulated to adopt and domesticate these concerns in their own work, one wishes for a rather less general-purpose plan. Political theorists, for example, however convincing they find Lane's accounting for the students' ideas, are not likely, without special coaching, to be spurred to a similar attack on formal systems of political philosophy—the gap is simply too wide. Psychiatrists, for their part, will hardly be won to the study of patients' preconscious idea-systems by an analysis so sociological, and where psychiatric, coarsely Adlerian. Political sociologists seeking to use these cases (to illuminate political recruitment and "socialization"?) will regret the absence of crises or causes in these drab

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Eisenhower days to ruffle students' smug self-enclosure and the lack of attention to the impact of college. Political psychologists, desperate for well studied lives, will deplore the bundling of cases by theme and the withholding of protocols (one has even to *index* subjects oneself) as well as an odd incuriosity about the childhood and infantile prototypes of outlooks (one man is allowed to boast, unreproved, "Not one of my present opinions is more than a year old"). Finally, what is the place of "guided autobiography" in politics teaching? The implicit case is strong, but quite undeveloped.

Many lively, even playful, passages, however, grace the rather sombre colonnades of *Political Thinking*. Take the pages on what being fat, or good-looking, or precociously intellectual, does for a man's politics, or those assessing subjects' moral sensitivity (he finds a link between religious orthodoxy and moral "primitivism"), or the deeply ironic catalog of admirable mental and moral qualities which, if taken to excess, work only to paralyze the political will.

We learn mostly by following Lane's expectations of his men—secure in his intimate grasp of their social backgrounds and campus and career ambitions. But we learn even more when these are disturbed, e.g. when the most personally insightful turn out to be somewhat crude analysts of politics and society; or when those most closely conforming to their parents' politics are found neither dependent nor passive—"I now see that sharply cutting the cord is not necessary for independence." The young man's need to continue, yet reshape, the family outlook is fully and compassionately treated. I suspect, though, that the "anxious college liberal and his need to be liked" may turn out to be principally an American type. Henry Murray's dozen war-shy but guilty Harvard men —the only other student group to have had their outlooks subjected to tough psychological scrutiny—were, it must be said, humanly a rather more attractive lot than these "courageous and insightful" young men.

The Caucus Papers are a welcome collection, admirably wideranging (three each on theory, domestic politics, U.S. foreign policy, and the "profession"), but somewhat lacking in force, coherence, and, to be blunt, talent. The editors themselves apologize for so much about the agreed and obvious flaws of "pluralism" and so little about (let alone deploying) new perspectives or modes of analysis. A "social science of politics" is about as vague and timid a radical aspiration, surely, as it might be useful to have. Foreign readers will sympathize with the strictures on premature and empty "professionalization" in U.S. political science) but students' credulity and care for the air-conditioned career must over the years take much of the blame. They will be less patient with griping about how hard it is to get a good education in a world uni-

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versity. Anthropologists have long known it is themselves they must learn to use as their principal instrument of analysis. In this volume only David Underhill's participant-observer account of the Columbia "events," moves a little in this direction. Let us hope for larger strides in Caucus Papers II.

A. F. DAVIES
University of Melbourne

Migration *edited by J. A. Jackson. New York, Cambridge University Press, 1969.—vii, 304 pp. $10.50.*

This volume contains ten articles on migration written mainly by sociologists. Its purpose is to review contemporary migration theory in the social sciences, to provide a basis for evaluating this theory, and to contribute to migration theory. However, the book does not fully achieve these goals. It provides a useful review of the sociological writings pertaining to migration, but migration research in other fields in general is not adequately surveyed. Charles Price's review of assimilation literature and theory is excellent and unique, and Clifford Jansen's surveys of work on the sociological aspects of migration is interesting. As for providing a basis for evaluating migration theory or contributing to migration theory, relatively little attention was devoted to these goals. Harold Lind, however, did test economic theories of migration in Great Britain, and R. C. Taylor tested some sociological ideas relating to migration in West Durham. Everett Lee's piece entitled "A Theory of Migration" was reprinted once more and is one of the best essays on the subject.

The chief weakness of this anthology is its strong sociological bias. Even though two economists contributed articles and there are frequent claims that the approach is interdisciplinary, most of the articles are parochial in that they are heavily slanted toward sociology. This bias, however, is not characteristic of the better essays in the volume. In his editorial introduction, J. A. Jackson calls for a sociological model of migration, and this notion is found in many of the other essays. I think that, instead, the emphasis should have been on a model or theory of migration that is interdisciplinary and related to general demographic theory. Few of the authors emphasize in any way the basic interrelationships of demographic variables. For example, the interaction of the increase in the native work force (which, of course, is related to natural increase, which, in turn, is affected by fertility and mortality) with the increase in job opportunities and migration would seem to be a very basic interrelationship.

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Despite these limitations, this is an interesting group of essays and should be read by anyone interested in migration.

ROBERT A. LEWIS
Columbia University

Respectable Radical: George Howell and Victorian Working Class Politics *by F. M. Leventhal. Cambridge, Mass., Harvard University Press, 1971.—xviii, 276 pp. $7.75.*

The Victorian working-class home had little room for the storage of papers and records, and it was most unusual for a leader of the nineteenth-century British labor movement—unless he was a middle-class sympathizer—to be able and willing to preserve any substantial proportion of the records of his career. The obvious exceptions are George Howell, whose collection is at the Bishopsgate Institute, and John Burns, whose diaries and correspondence are in the British Museum. Howell, born and brought up in Somerset in the early Victorian years and later occupied as a bricklayer in London, became active in the movements which led to the Reform Act of 1867 and to the establishment of the Trades Union Congress, of which he became the first secretary. Like most of his generation of trade-union politicians, he was a member of the Liberal party and became Liberal member of Parliament for Bethnal Green in 1885—a seat which he retained for ten years. He never joined the Labour party, which in any case was only formed in 1900.

Howell was a hard-working, respectable, but distinctly prosaic figure, and Dr. Leventhal seems to have found him rather unattractive. Nevertheless he has written a biography much of which Howell himself would have approved, for it recounts the story of his youth and early middle age in painstaking detail and is evidently soundly based upon the available evidence, much of it provided by Howell himself. The most interesting section to those already familiar with nineteenth-century British labor history is the account of the Somerset and early London years. This is based on Howell's own memoir, and we obtain from it a vivid picture of the precarious character of working-class life and of the difficulties of self-education even for a most determined and intelligent young man. When the author plunges into the story of Howell's political and trade-union activity in the 1860s and 1870s, the detailed narrative drags somewhat, and the reader longs for some picture of Howell's personality or of the conditions in which his life was led. To be sure, students of labor politics will find these narrative chapters to be of value in enabling them to understand more fully and to separate the various strands of agitation

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which intertwine in this period. But there is no extended reassessment of Howell's role or defense of his political views against the later critics who regarded him as insufficiently militant. Perhaps Dr. Leventhal agrees with the critics, but if so he should say why.

From the 1880s onward, the author seems to lose interest altogether, presumably because he thinks that Howell's centrality in labor politics is at an end. Howell's ten years as a member of Parliament, together with a further fifteen years before he died, are dealt with in the last ten pages of narrative. Yet in this period, Howell wrote several books and many articles, and although he disagreed with the views of younger labor leaders, he found many of his own generation to approve his sentiments. Dr. Leventhal seems somewhat unfamiliar with the politics of the period at the turn of the century and only anxious to bring his book to an early close. He accepts the view that Howell was no longer a representative figure in this period, but such a view deserves fuller consideration and reassessment in a biography, and it is difficult not to believe that the author is being less than fair to a man whose earlier career at least entitled him to make an informed judgment of the fresh developments of the late Victorian and Edwardian years. The hasty conclusion mars an otherwise workmanlike biography.

HENRY PELLING
St. John's College, Cambridge

Conscience and Politics: The British Government and the Conscientious Objector to Military Service, 1916-1919 *by John Rae. London, Oxford University Press, 1970.—xvi, 280 pp. $11.25.*

In his treatment of a highly emotive subject, John Rae has steadfastly resisted the temptation to indulge in histrionics of one variety or another. For this, quite as much as for his skillful use of primary source materials, he deserves his readers' respect. Conscientious objection in the First World War, as the author modestly puts it, "constituted a minor but intractable problem for the British Government" (p. 1). Nonetheless, it was one that shed significant light upon ministerial attitudes, bureaucratic procedures, popular sentiments, and the underlying controversies regarding the general conduct of the war.

Unlike his predecessors in the field, whose studies were stronger in moral rhetoric than in thoughtful scholarship, Rae eschews facile condemnation. That the "conchies" suffered ridicule and injustice cannot be denied. But, as Rae convincingly demonstrates, their unhappy fate was, more often than not, the product of mutual misapprehension.

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Sometimes, too, they actively courted the abuse that their compatriots obligingly showered upon them.

Proceeding hurriedly and with due concern for political exigencies, the authors of the 1916 Military Service Act had grievously failed to anticipate the nature, let alone the extent of the problem of conscientious objection. Furthermore, they were too much inclined to interpret it exclusively in administrative terms. The provisions that were made for conscientious objection, relatively generous by the standards of any day, in fact owed less to Liberal principle, as is commonly supposed, than to political expediency. Asquith, "the indispensable man" as he was regarded by himself and by a yet undiminished segment of his countrymen, sought to mollify his Radical backbenchers, who were justly incensed by his abandonment of voluntary recruiting. By conceding an exemption to military service on grounds of religious or other beliefs, he could appeal to recalcitrant trade unionists as well as various nonconformist sects.

Although the prime minister and his Cabinet colleagues, Walter Long chief among them, acted in the best of faith, their procedures were never as clear as their intentions. Those who applied for status as conscientious objectors were required to appear before local tribunals, bodies which had been created to perform altogether different services and which varied enormously in their ability to respond to the demands placed upon them. We are, thank goodness, spared another rendering of Lytton Strachey's tribulations, but Rae provides ample evidence that the system could be partisan and vindictive. Readily acknowledging that various tribunals "made errors of judgment and administration, and that these errors were sometimes inspired by prejudice," he remains convinced that, "in the context of conscription as a whole, the limitations of the tribunal members seem relatively unimportant" (p. 94).

Rae is particularly careful to distinguish between the various grounds of opposition to military service—political, intellectual, and eschatological. While his references to the UDC are perplexingly few and those to the Bloomsbury set tantalizingly brief, he excels in his analyses of the Quakers, the Christadelphians, the Seventh Day Adventists, the Muggletonians, and, further down the line, the Dependent Conklers. Perhaps his most original section is a detailed investigation of the so called absolutists, a group less impressive numerically than intellectually, who incurred heavy prison sentences by their categorical refusal to serve on any basis. The most celebrated of the absolutists, at least in that capacity, was Stephen Hobhouse, on whose behalf a vigorous campaign was waged by an assortment of prominent and highly dissimilar political figures, many of

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them ardent conscriptionists.

It is with regard to the Hobhouse case that Rae is most successful both in narrating the events and in blending the two components of his title. On the whole, questions of conscience are divorced from those of politics, and the latter are not always sufficiently brought to bear on the former. It is, for example, difficult to gauge the deeper significance of the Lloyd George coup. Asquith, who is treated brusquely in the opening chapters, gradually acquires a moral courage (possibly with Gilbert Murray's assistance) that seems to surprise the author as much as the reader; Lloyd George, on the other hand, is invariably credited with a shrewdness that his actions do not always appear to justify. As Rae tardily concludes (p. 245), it is the relationship between "conscience and government" with which he is most specifically concerned. On this score, he has made a solid contribution, but one more limited than his ambitious title promises to deliver.

STEPHEN E. KOSS
Columbia University

The Napoleonists: A Study in Political Disaffection, 1760-1960 *by E. Tangye Lean. New York, Oxford University Press, 1970. —xiv, 402 pp. $12.75.*

Some books are dull. Some books are interesting. Some books suddenly become interesting half-way through. This is one of the last kind. At first sight, the principal subject seems to be a survey of Napoleon's sympathizers in England in the early nineteenth century. A fine subject, but the early chapters, though quite evidently written from thorough knowledge and research, are difficult to master. There are biographical vignettes of figures such as Mrs. Inchbald, Thomas Whitbread, and Lord Byron; there is a lengthy discussion of the building and settlement of Edwardes Square, in the neighborhood of Kensington Palace and Holland House, and of its residents who included many persons out of sympathy, for various reasons, with the establishment in church and state; and there is a detailed account of the lengths to which ardent admirers of Napoleon, such as Lord and Lady Holland, went in order to achieve and maintain contact with him in exile, first at Elba and then at St. Helena. All this is potentially full of interest. But one occasionally gets the feeling that it does not hang together properly, that the book is going to turn out to be a slightly confusing hodgepodge of assorted anecdotes and incidents.

Then, suddenly, with the section entitled "The Napoleonist Syndrome,"

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the book springs to life. For here it becomes clear that Mr. Lean is not merely concerned with depiction and description, but also with the attempt to answer the problem as to whether those who sympathized with Napoleon, even when (or, perhaps, particularly when) he was threatening their country, may be said to belong to a definable type of personality. Was there anything in common between Byron and Whitbread, Charles James Fox and Lady Holland, William Godwin and Mrs. Inchbald? Were there certain childhood experiences which bound together the disaffected Napoleonists? Mr. Lean answers both these questions in the affirmative. On the social and political level, the Napoleonists tended to share a background of nonconformity, religious or otherwise, and of sympathetic involvement on the side of the colonists with the American Revolution. On the psychological level, many of them had powerfully assertive fathers, against whom they could not revolt because it was morally impossible to do so, since the fathers themselves were unacceptable to the community at large, and thus had to be defended and protected. By taking Napoleon's part, the Napoleonists were, in fact, siding with their own ancestry "in the wilderness" and thus proving their loyalty to their fathers; "but at the same time, and under cover of this loyalty, they allowed themselves to denounce what was unacceptable in their parents by displacing it on the center of the country" (p. 221).

It is the manner in which Mr. Lean expands and supports this thesis which lends depth and excitement to this book. One may not always agree with him, especially when he tries to extend the thesis to twentieth-century "Napoleonists" such as Lloyd George, Mosley, Philby, and Burgess. But he has opened up a fascinating subject for study in an original way. And the illustrations are nothing less than superb.

JOHN CLIVE
Harvard University

Jews and Freemasons in Europe, 1723-1939 *by Jacob Katz, translated from the Hebrew by Leonard Oschry. Cambridge, Mass., Harvard University Press, 1970.—viii, 293 pp. $11.00.*

This volume by Dr. Katz, professor of sociology and rector of The Hebrew University of Jerusalem, is part of his long-term study of the emergence of the Jews into modern society and of the pressures this created among Jews and within the various national societies of which some Jews at least sought to become fullfledged, respected members. It was be-

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gun at Harvard University in 1962-63 when Dr. Katz was a visiting professor there and was able to make use of its splendid resources on this question, including the great collection given to Harvard by Lee Max Friedman. It was completed after Dr. Katz had studied in most of the great libraries of Western Europe and had had the opportunity also to work in the Masonic archives in Berne, Zurich, Frankfurt, Paris, and the Hague, the last of which were of especial value. However, for unexplained reasons, the United Grand Lodge of England denied him access to its archives, a serious loss because of the central role English masonry played, especially in the eighteenth century.

Unfortunately, the volume does not contain a bibliography or a bibliographical essay, which would have particular value for a subject of this kind. Moreover, the 51 pages of notes are all concentrated at the end of the volume.

Dr. Katz notes that Jews began to move into modern society from their ghettoes at about the same time that the Masonic order appeared and grew, with the principles incorporated in the first Masonic constitution of 1723 including that of religious toleration. His book therefore devotes substantial attention to the efforts of Jews seeking full membership in their national communities to join Masonic societies, some of which, especially in Prussia, refused to honor their own principles of toleration. He describes in some detail the conflicts within and between various Masonic lodges over Jewish membership, showing clearly that many Masons who sought freedom for their semi-clandestine societies and beliefs were as unwilling as their fellows outside the lodges to grant those same rights to Jews. After reaching the peak of acceptance in lodges in the 1860s and 1870s, the Jews began to face the effects of rising nationalism throughout the West. This led to tightened restrictions against them within the Masonic lodges. At the same time, in a great irony, the new nationalists lumped together the Jew and the Freemason as pernicious secret agents trying to establish a Jewish-Masonic domination over the Christian world. This battle cry helped lead to the Dreyfus Affair in France. Later, of course, Hitler and the Nazis made use of the same charge, which led to the end of Masonry in Germany in 1935 and to the holocaust soon after.

This is a fascinating subject, and Dr. Katz by his use of the Masonic archives has shed new light upon it. However, he concentrates almost entirely upon Germany; he provides little analysis of the social groups who joined, or sought to join, the Masons at various times during the two hundred years under review; and he neglects a good deal of the signifi-

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cant literature on the anti-Masonic movement in the twentieth century, particularly in countries other than Germany.

ROBERT F. BYRNES
Indiana University

The Lysenko Affair *by David Joravsky. Cambridge, Mass., Harvard University Press, 1970.—ix, 459 pp. $13.95.*

This is an important study of 35 years (1929-64) of Soviet political history focused on the crisis in agriculture, the rise to power of the peasant agronomist Trofim Denisovich Lysenko, and the collapse of his influence when its political props were removed by the fall of Khrushchev. The additions which it makes to knowledge of that period derive from an examination of published sources in the USSR and the U.S. which appears to be exhaustive. The bibliography, the extensive notes containing references in full to all articles cited, the lists of persons arrested or otherwise suppressed, together with a full index, occupy a third of the pages. The whole body of documentation in the public domain seems to be set forth for the first time for this period and for this problem, probably as complete in respect to significant items as it is likely to be until the locked archives of the Soviet state are opened to scholars.

The remaining two-thirds of the book is devoted to a readable narrative and commentary, with interpretation, of what the record reveals concerning the interaction of agricultural practice, natural science (especially biology), ideology, and political power in the turbulent period following Stalin's consolidation of his power. Since this book follows by eight years Professor Joravsky's *Soviet Marxism and Natural Science, 1917-1932*, the author has now reflected both upon the origins and outcomes of relations of science and Marxist-derived political theory and practice. The present book greatly expands this purview not merely in years but in depth and in breadth of confrontation with major historical problems.

The chief problem addressed is the relation between ideology and practical policy. Specifically, in the author's words, "By what mechanisms of institutional development was the Soviet campaign for modern farming plunged into tyrannical irrationality and then extricated from it?" (p. xi). The Lysenko affair is of course more than a pretext for examining a more general question. It has a bizarre, even gruesome interest of its own. Ignorance, in the malignant form of anti-intellectualism and anti-scientism, dominated agricultural policy of a modern state for thirty long years, causing incalculable harm to productive capacity, result-

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ing in the arrest and execution of scientists and administrators of agricultural programs, and turning back the clock of technical progress in agriculture at a time when in other countries it was being accelerated. How could this happen, and especially how could it continue for thirty years, even reaching its climax just at the moment (1948) when the nation was rising from the ashes of a disastrous war?

Explanations of this remarkable fact have been offered in the many accounts of the rise of Lysenko by non-Soviet writers, but not until the publication in English in 1969 of *The Rise and Fall of T. D. Lysenko* by the Soviet biochemist Zhores A. Medvedev and of the book under review has it been possible to justify any of the explanations by reference to the Soviet documents. Joravsky, with better access and more thorough use of the historical record than Medvedev, has now pretty well disposed of earlier interpretations. The rise of Lysenko was not earned by any contribution to science or to practice on his part, for he made none. The suppression of genetics was a result of Lysenko's influence, but no "Lysenko genetics" filled the gap, nor was justification by Marxist theory or "dialectical materialism" the cause of the long dominance of Lysenkoism.

Two other agricultural cults had already established themselves in popularity, with the approval of party leaders, before Lysenkoism developed. One had its origin before the revolution. Its leader was Michurin, a breeder of fruit trees who claimed to have discovered new methods of "training" grafts, which subsequently proved to have no scientific basis. The Lysenko school adopted some ideas of Michurin, who before his death in 1935 had become a popular hero, something like Luther Burbank in the United States.

The other cult was led by an old agronomist, Vasili Robertovitch V'ilyams, the son of an American engineer, Robert Williams, who had settled in Russia in the nineteenth century. Some of V'ilyams's views were compatible with those of Lysenko. He was *persona grata* with the party as an early convert to Bolshevism. Lysenko fell heir to the ill-founded prestige of these two other "practical" authorities, and by continually expanding the domain of his proposals for "conquering nature" to the planting of grass and of trees, and eventually to the breeding of cattle, reached the peak of authority as president of the Lenin Academy of Agricultural Sciences and director of the Institutes of Genetics and of Plant Breeding. He was thus for many years the arbiter not merely of the practical but of the scientific development of agriculture, which meant that his decisions would govern not only for the short but the long haul. But both failed. The one that caught up with him was, of course, the

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short haul. Agricultural production did not increase *pari passu* with effort or investment or any other input, and one day in 1964, Khrushchev, the boss who had inherited this liability from Stalin, was called in by the Central Committee and summarily sacked. The same drain carried Lysenko and his nostrums away, although somewhat more slowly, a matter of months rather than days. Thus was applied Stalin's "Criterion of Practice." That was, and is, the final arbiter in the Soviet state, perhaps in any state.

The rise (to about 1953) and the more gradual fall of Lysenko affected, as well, his supporters who included politicians, agricultural technicians, and economists, and even some compromising scientists in the earlier period, but never the peasants to whom the programs had been intended to appeal. This history is set forth in detail in the central section of the book, with names, dates, and fates of the actors and victims. The latter included many scientists. It is a reminder that history does not express the operation of reason to reflect that Lysenko's name may outlive in history the names of Chetverikov, the founder of population genetics, who died in poverty, blindness, and exile; of Vavilov, a founder of modern plant breeding who died of malnutrition in prison; of Karpechenko, Kolzov, Levit, Levitskii, Serebrovskii, Timoféeff-Ressovskii, and many another who paid with his life or his career for opposition or failure to follow a lead that proved false for science, for practice, and for the good of the Soviet state.

In the final short chapter, in my opinion the best in the book, "Ideologies and Realities," the author reflects on what has gone before. The Stalinists were anti-intellectual, ideological fanatics. Ideology as an influence in the Soviet Union was not only "unacknowledged dogma that serves social functions." It was also rationalization of what the big boss thought. It includes what V. Stefansson many years ago called "the standardization of error." In the Lysenko case, this led for a period of 35 years to the rejection of truths of agricultural science and the substitution for them of the nostrums of cranks, untested until they failed in practice: "The Stalinists intended to raise farming to the level of science, but they accomplished just the opposite, dragging science down to the primitive level of peasant farming, and ultimately produced a completely unintended restriction of political power" (p. 311).

The political bosses learned their trade slowly and at great cost to the people: "Perhaps the most valuable function of the Lysenko affair, like a disease that immunizes, was to generate self-restraint and rationality in the ruling class." This is probably a statement of hope rather than a rea-

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soned conclusion, for the author concludes the account of his ten years of study of this affair with an appeal to the Communist vision of Mayakovsky and the Christian faith of Pasternak. This is hardly an outcome of experience or even of "faith in the liberating power of science." The reality with which to balance ideology has unfortunately to be sought in the real world—now. Poetry may be its solace, but the problem remains.

Nevertheless, as a natural scientist interested in history, I take it as a sign of good faith in an historian dealing with recent events that he does not try to substitute for faith and hope, a claim to "establish causal relations" among historical processes. To view such sequences as "established" would regard the relevant historical problems as settled, a result as deplorable for historical research as it has proved to be for science.

On the contrary, the questions at issue in this book, which deal with basic processes of political behavior as adaptations to changing circumstances, are full of the interest of active life. They are thus presented as they rightly should be, in a lively manner in which the personal opinions of the author are allowed on occasion to play their part. Complete objectivity which seems a desirable and normal attitude in natural science, is less so, if attainable at all, when dealing with the results of fallible and often irrational human actions. This fine study should be enjoyed therefore by scientists of diverse kinds, and by a wider public as well.

L. C. DUNN
Columbia University

Struve: Liberal on the Left, 1870-1905 *by Richard Pipes. Cambridge, Mass., Harvard University Press, 1970.—xiii, 415 pp., $10.00.*

Professor Richard Pipes's study of P. B. Struve's life and thought up to the Revolution of 1905 is an expert piece of work. Based on 301 items, not counting newspaper articles, published from 1892 to 1905 by that remarkable Russian intellectual, on considerable archival materials, especially letters, and on various other sources, the book represents a rich, full, and one suspects largely definitive, if not exhaustive, account of Struve from 1870 to 1905. Those Soviet archives which Professor Pipes was not able to see, as well as other material which might eventually become available to scholarship, are not likely substantially to change the picture. The essentially chronological narrative consists of fifteen chapters grouped into three parts: implanting social democracy, the united

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front, and liberation. It is followed by a very valuable bibliography and an index. The volume also contains a number of excellent photographs of Struve, his family, and his associates.

In presenting a clear and careful account of Struve's early life, Professor Pipes deliberately treats his hero from the outside. He attempts no psychoanalysis or other far-ranging interpretations of character and behavior. Instead he concentrates his analytic powers together with the great bulk of his attention on Struve's thought. Nationalism, inspired in part by Ivan Aksakov, formed its earliest and most fundamental substratum. Liberalism and positivism followed. Socialism, by contrast, never acquired, according to Professor Pipes, a comparable standing in Struve's mind and heart and could therefore be effectively abandoned in his later life. The real change in Struve's orientation occurred in the autumn of 1900, when "he gives up positivism as an all-embracing system, and accepts a dualistic philosophy with two parallel realms, one empirical, the other transcendental or metaphysical" (p. 297). It is in the penetrating depiction of Struve's intellectual evolution against the background of Russian thought and radical politics that Professor Pipes achieves his best results.

But the book has interest even beyond the important figure of Struve. In the words of the jacket blurb, for once correct: "Much attention is given to such movements as 'Populism,' Marxism, and Liberalism, each being given a somewhat different interpretation from that prevailing in the historical literature." Not all interpretations are entirely convincing. One might take exception, for instance, to Professor Pipes's image of Lenin, including the assertion that no trace of Oedipal complex can be found in his biography (incidentally, according to Professor Pipes, there is no indication of that complex in Struve's biography either). And even the few admirers of Russian liberalism might not agree with Professor Pipes—and Max Weber—that "Russian liberalism was a movement not of socioeconomic interest but of ideas," that its main supporters from the educated gentry had nothing to gain and much to lose from the application of its principles in Russia. Yet, even when not compelling, Professor Pipes's rich commentary on the Russia of Struve's day is usually learned, intelligent, and suggestive.

Students of Russian history are indebted to Professor Pipes for this first volume on P. B. Struve and will look forward to the appearance of the second.

NICHOLAS V. RIASANOVSKY
University of California, Berkeley

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The Origins of Franco's Spain: The Right, the Republic, and the Revolution, 1931-1936 *by Richard A. H. Robinson. Pittsburgh, University of Pittsburgh Press, 1970.—475 pp. $9.95.*

During recent years, the political and social antecedents of the Spanish Civil War have come to be viewed in a fresh manner. Edward E. Malefakis's study of the implementation of the Agrarian Reform Law of 1932 and Stanley G. Payne's comprehensive survey of the revolutionary Left have done much to modify the formerly guiltless image of the forces which backed the Popular Front. This latest book by Professor Richard Robinson of the University of Birmingham adds further strength to the revisionist trend by taking a new look at the opposite end of the political spectrum—the parties of the Right. The main value of the study derives from its comprehensive treatment of the full diversity of forces on the Right and not simply those groups which actively supported the military uprising. In so doing, Professor Robinson is able to show that a sizeable sector of the Right followed a possibilist course with respect to the republican regime and accepted the ultimate holocaust only with extreme reluctance. Particular emphasis is laid on the role of the CEDA, the Social-Catholic party formed in 1933 which quickly emerged as the strongest force on the Right and the lone political organization outside that of the Socialists which could truly boast of a mass following. Recounting the development of the party through press accounts and the published speeches of its leaders, Professor Robinson concludes that the CEDA was much less of a threat to the established political order than its opponents on the Left portrayed. That is, although their ultimate goal was a conservative regeneration of Spanish society through the establishment of a corporative state, the CEDA's leaders were willing to pursue these objectives by the means provided by the existing parliamentary institutions and on an evolutionary basis. Such a willingness to exploit the existing opportunities for organized political activity set the CEDA at an immediate advantage over the competing monarchist factions, whose energies were dissipated by internal bickering and fruitless conspiratorial ventures. However, although the CEDA achieved spectacular success in the initial mobilization of conservative sentiment, it never stabilized itself as a full Christian Democratic party. Professor Robinson cites internal differences on the issue of social policy and the disruptive influence of the party's youth movement as partial explanations for this failure, but his main emphasis falls on the paranoid reaction of the Spanish Left. By refusing to accept the CEDA and allowing their fears to dictate excessive actions of their own, the Spanish Left undercut the principal force of moderation

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on the Right and drove conservative opinion in the direction of totalitarian reaction.

The end result is a book which makes a substantial contribution to a fuller understanding of the origins of the Spanish Civil War. Although specialists in the field will not be surprised by Professor Robinson's sympathetic portrayal of the CEDA, the point that a sizeable current of possibilism existed within the Spanish Right deserves the full emphasis which it receives in this study. The author has assembled much useful material on the diverse parties and movements comprising the Right, particularly with respect to the CEDA whose history has been inexplicably ignored up to this point. A weakness of the study is that Professor Robinson's treatment of the CEDA leaves unanswered many questions concerning the party's internal structure which undoubtedly had a great bearing on its ultimate failure to stabilize itself as a full Christian Democratic party and to provide the much needed conservative bulwark to the republican regime. Even more serious is the author's failure to relate the ideologies of the competing political movements to the economic and social conditions in which they developed. By ignoring the social and economic context in which the confrontation of Left and Right took place, Professor Robinson has tended possibly to overemphasize the availability of the Right to accommodation while setting the extremism of the Left in an unduly harsh light. At any rate, Professor Robinson has written a useful book, one which at once excites controversy while providing a substantial body of material for future research.

GARY E. GEISLER
University of Cincinnati

France and the United States: Their Diplomatic Relations, 1789-1914 *by Henry Blumenthal. Chapel Hill, The University of North Carolina Press, 1970.—xvi, 312 pp. $9.75.*

De Gaulle and the Anglo-Saxons *by John Newhouse. New York, The Viking Press, 1970.—ix, 371 pp. $8.50.*

The Foreign Policies of France, 1944-1968 *by Guy de Carmoy, translated by Elaine P. Halperin. Chicago, The University of Chicago Press, 1970.— x, 510 pp. $15.00.*

The first of these books is an appropriate introduction to the other two, which cover much of the same ground. A gap remains, from 1914 to World War II, a period during which Franco-American relations had not a little importance. To be sure, these relations were not the exclusive

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focus of French foreign policy, but certainly, especially in view of the intimate Anglo-American relationship, they loomed large on the French political horizon.

Over the longer term, Franco-American relations have been characterized by what may be called a low profile.. France loomed quite large in the rise of the American nation to independence, but, in that case as in many others, alliance, a conjunction of interests, must not be confused with friendship. Still, the myth of Lafayette dies hard.

But affective relations are unstable. Although France enjoys with Russia the distinction of being the only major power with whom the United States has never engaged in open hostilities, hostility of feeling has been very real at times. The French revolutionary period and France's Mexican adventure, to cite but two illustrations, gave rise to strong reactions in Washington. But perhaps the fundamental fact is the relative lack of American participation in the international politics of the nineteenth-century great powers. Awareness of the American potential is rarely found in France and when found is apt to be expressed in resentment. That is the tale that Blumenthal records, with a wealth of documentation, in balanced fashion on the whole, though French imperial activity could have received more adequate treatment.

The passing of General de Gaulle has put a close to a stormy career though not an end to the debate that will long flourish around his controversial figure. Newhouse's treatment is highly critical, to the point at times of suggesting the question, how could a man so wrong, high-handed, and inconsistent partake of the attribute of greatness? Grounds for criticism indeed abound, but the mass of detailed information could have been organized into a more coherent whole, not to mention the fact that on occasion the source of information would be welcome. The closing pages make good reading.

What can a country like France, in 1945 technically victorious but in reality defeated, do but seek to recover its position? The question however is, How? The analysis of the methods used to achieve that end is the unifying thread that runs through de Carmoy's detailed survey. Here also we have a mass of information, with perhaps some avoidable repetition.

At the end of the war, de Gaulle dominated the French scene; he did again for a decade after his return to power in 1958. The intervening gap, the twelve years of the Fourth Republic, is examined in equal detail. After the war, as before, security loomed large among France's desiderata, but after 1945, in contrast with the period after 1919, France was not in a position to assert her will; the European, like the world, scene was dominated by the superpowers. Within narrower limits of maneuver the

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Fourth Republic, mindful of the failure of France's interwar policy, produced the imaginative solution that was the integration of free Europe. Britain having failed to seize the opportunity to lead that movement, the result was a potential French leadership, a leadership, moreover, acceptable to others.

But the Fourth Republic, which, in addition, laid the bases of economic recovery and modernization, labored under two handicaps. One was the confused operation of French politics, government by assemblies; the other, the inability to deal with the imperial problem. It was the imperial problem, more specifically the Algerian rebellion, that brought down the Fourth Republic and put de Gaulle once more in the position of savior of France.

De Gaulle, in many ways a modern and forward-looking man—witness his prewar writings—entertained at the same time a romantic vision of France. The result was a return to the harsh operation of power, the Richelieu tradition, an illustration of the quip that *Realpolitik*, a German word, can be a French practice. His goals, pursued with dogged persistence, were two: independence and primacy in Europe. His problem was how to accomplish these ends from a position of weakness. The answer was his policies and his methods, of which de Carmoy is highly critical, contending, with not a little foundation, that the only possible way for France to achieve primacy is through the integrating solution that the Fourth Republic had launched.

Quite appropriately, the second "reign" of de Gaulle is divided into two parts, with the year 1962 as the watershed. Up to that time, monetary management and the liquidation of empire were handled in masterful fashion. But thereafter things went wrong. Having, or thinking he had, established a solid basis of power, de Gaulle launched into the impossible task of countering American power and influence, with the consequence, in effect, of abetting the opposite of what he really wanted, French independence and primacy.

The events of 1968, in France itself and in Czechoslovakia, effectively destroyed the Gaullist illusions; yet, de Gaulle's capacity to think "unthinkable thoughts" might also be remembered.

René Albrecht-Carrié
Rockford College

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Book Notes

National Interest (New York, Praeger Publishers, Inc., 1970; 173 pp. $5.00) by Joseph Frankel, is an interesting little book. It contains two main parts which are devoted to the meaning of the concept and the formulation of national interest. The book has the merit of dealing with something which, under the headings of *raison d'état* or national interest, was and is unavoidably present in the minds of all the political decision makers. In our time President de Gaulle, for example, referred only to his country's national interest to justify France's withdrawal from NATO and France's attitude toward the Middle East conflict; more recently, President Pompidou declared that the reintegration of France into NATO would be contrary to his country's national interest. These references to recent political facts lead us to point out that the book has the merit of giving numerous examples drawn from contemporary politics. The author often applies himself to choosing examples from the foreign policies of Western, Communist, and emerging states. The approach, however, is chiefly synthetic. Professor Frankel proposes, for example, the interesting distinction between the "aspirational level" of national interest, which refers "to some ideal set of goals which the state would like to realize if this were possible," and the "operational level," which refers to the sum total of interests and policies actually pursued" (p. 31). The instability in the definition of a particular state's national interest originates, says Frankel, in an excessive tension between these two levels. The major drawback of the concept is that it refers to things that are as different as, for example, strategical territories and commercial markets; hence the interest of chapters four and five which deal with the "Dimensions of National Interest." These are, according to Frankel, the strategic, political, and economic dimensions and the dimensions of time and space. Only the first three dimensions seem related to the very

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content of national interest and seem therefore the only real dimensions of it. The ideological dimension could be added to these real dimensions. Finally, it is perhaps regrettable that the discussion of the dimensions of national interest has not led the author to suggest some ultimate concept such as that of a "political project." Indeed, national interest (at the "aspirational" and "operational" levels) is something which is in conformity with the *raison d'être* of a political community, i.e., its "political project," which is made up of the particular purposes of ensuring national security and civil order, securing prosperity, safeguarding and promoting characteristic national values, and developing or keeping up internal solidarity or national consciousness.—JEAN BARREA, *Université Catholique de Louvain.*

Modern Bolivia is a country poor in almost every resource except the variety of its political traditions. In *Parties and Political Change in Bolivia, 1880-1952* (Cambridge, Cambridge University Press, 1969; xv, 451 pp. $14.50), Herbert Klein has provided an authoritative study of the background to one of the most important of Bolivia's historical traumas —the revolution of 1952. Why should this backward country have undergone a social and economic revolution more thoroughgoing than any previous one in Latin America except the Mexican? Although some historians may argue that the ultimate answer lies in the singularity of the Bolivian past, Professor Klein offers much evidence for the student of comparative revolution. Defeat at the hands of Paraguay in the Chaco War (1932-36) cost Bolivia a large block of territory and an even greater blow to the pride of her elite. It was a watershed in the development of the national consciousness. The "generation of 1932" turned its energies to the restructuring of the country, still an overwhelmingly agricultural economy dominated by a small elite that took little interest in the Indian masses. Relying primarily upon Bolivian newspapers and secondary works (no personal or institutional archives are cited), Klein skillfully traces the turbulent succession of governments and political realignments during the 1930s and 1940s. It was this "fractioning of the traditional system," according to the author, that made social revolution "almost inevitable." But also of crucial importance was the manner in which the military officer corps had become politicized. The Left could come to power only because the conservative forces had already been neutralized by the "military socialism" of Colonel David Toro and Colonel Germán Busch. Although their reformist policies proved contradictory and incomplete, they committed the military to a social policy that rendered more difficult subsequent intervention on behalf of land-

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owners, mine owners, wealthy merchants, or foreign investors. Klein ably chronicles this military involvement in the nationalist and socialist trend, but he is never quite able to tell us why it developed, other than to argue that socialism was a useful device to divert civilian attention from the military debacle in the Chaco War. There remains also the question of why the middle classes proved such loyal supporters of revolutionary policies. Klein offers valuable evidence on the radicalization of the student and trade union movements and the ineptitude of the moderate Left, as well as the effects of inflation in 1949-50. Yet the variety and strength of the Bolivian revolutionary Left seems inexplicably great, especially when compared to other Latin American nations. Finally, one might question the author's remarkably unilateral interpretation, which focuses almost exclusively on political factors. Nonetheless, Klein has written a valuable synthesis which will prove indispensable to all students of modern Bolivian history.—THOMAS E. SKIDMORE, *University of Wisconsin.*

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In Memoriam

PHILIP EDWARD MOSELY

The Faculty of Political Science records here its sorrow at the loss on January 13, 1972 of Philip Edward Mosely, Adlai Stevenson professor of international relations, director of the European Institute, and associate dean of the Faculty of International Affairs. The chapter titles in any account of Philip Mosely's professional career would be easy to list—Harvard undergraduate and graduate student; pioneering researcher in the Soviet Union and in the Balkans in the 1930s; Cornell teacher; war-time policy adviser and government representative, notably as Ambassador Winant's associate in the European Advisory Committee; Columbia professor of government and director of the Russian Institute in a remarkable period of the flowering of that institute; director of studies at the Council on Foreign Relations; again Columbia professor and reinvigorator of the program of its European Institute; and committeeman *par excellence* in a host of public and private activities. But these chapter titles only begin to suggest the variety and richness of his contributions to the scholarly community which he so conspicuously ornamented.

There were many Philip Moselys as all those who attended the moving memorial services on February 8 came to understand better than they ever had before. Enormously knowledgeable, indefatigably industrious, incredibly generous in the support of colleagues and students, meticulously but sympathetically critical of the draft manuscripts of the legion of scholars and would-be scholars who sought his advice, performing gladly and with little apparent effort but in fact with a great deal of effort as one of the great academic job entrepreneurs of our time, occasionally accepting assignments which it was unrealistic even for him to expect to complete—this lovable and dedicated colleague devoted himself, not just uncomplainingly, but gladly, to a life of service.

Phil Mosely's published work was substantial, but his unique vocation for scholarship found its expression in the writings of his students and colleagues as often as in his own. In every aspect of his professional career—in the university, in government, in research organizations, in foundations, here and abroad—he raised the level of achievement of all those whose lives he touched. We in this faculty who have known and worked with Philip Mosely are all by that fact better scholars. We are also, we may hope and believe, better human beings.

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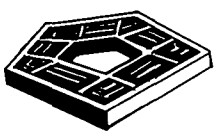

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